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# MIND

## A QUARTERLY REVIEW

OF

### PSYCHOLOGY AND PHILOSOPHY

EDITED BY

PROF. GILBERT RYLE

WITH THE CO-OPERATION OF PROF. SIR F. C. BARTLETT AND PROF. C. D. BROAD

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# M I N D

## A QUARTERLY REVIEW

### OF

## PSYCHOLOGY AND PHILOSOPHY

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### I.—EXPLANATION IN HISTORY

BY ALAN DONAGAN

FEW philosophers have accepted the opinion of the late R. G. Collingwood that 'the chief business of twentieth-century philosophy is to reckon with twentieth-century history'; but it is hard for anybody with even a superficial interest in what historians write to reject Collingwood's reason: 'about the end of the nineteenth century something of the same kind [as the seventeenth century revolution in natural science] was happening, more gradually and less spectacularly perhaps, but not less certainly, to history' (*An Autobiography*, Oxford edn. p. 79). But that it must be rejected, I shall argue, is a consequence of the theory of historical thinking which prevails among contemporary philosophers. This theory I shall call 'Hempelian', after Professor Carl Hempel, in whose fine essay, "The Function of General Laws in History",<sup>1</sup> its fundamentals were first comprehensively expounded. The name 'Hempelian' is used *honoris causa*, not because the prevailing theory derives from Hempel,<sup>2</sup> but because his essay is its classical statement, to which any variant may be accommodated as a gloss.

It would not follow that the Hempelian theory is mistaken, even if it should turn out to conflict with historians' or common

<sup>1</sup> *Jour. Phil.* vol. xxxix (1942), reprinted in Feigl and Sellars, *Readings in Philosophical Analysis* (New York, Appleton-Century-Crofts, 1949). Hereafter this essay is referred to as 'GLH', page references being to Feigl and Sellars.

<sup>2</sup> Most of the essentials of the Hempelian theory were developed as early as 1931 in the late Morris R. Cohen's admirable *Reason and Nature*, Book I, ch. I, sect. 2, and Book III, ch. I, sects. 1 (d), 2, and 3. A good select bibliography of periodical contributions may be found in Feigl and Brodbeck, *Readings in the Philosophy of Science* (New York, Appleton-Century-Crofts, 1953) pp. 797-798. This volume is hereafter referred to as 'FB'.

readers' estimates of recent historical work. No such estimate is immune from error : Berkeley's criticism of fluxions has worn better than the replies of contemporary mathematicians. Nevertheless, whenever the common opinion of those acquainted with a productive intellectual discipline conflicts with a philosophical theory, a sceptical re-examination of the theory is proper.

### (1) *The Hempelian Theory*

The cardinal tenet of the Hempelian theory is that all scientific explanations have a common form. A scientific explanation must be such that what is explained may be logically deduced from it ; a weaker connexion would invalidate it. There are two kinds of scientific explanation, differentiated by the kinds of thing they explain. The first of these is the explanation of individual happenings : a landslip, say, or a political murder, or a victory in battle. Explanations of this kind have two components : (1) statements of other prior or simultaneous happenings, and (2) general laws established by empirical evidence. General laws admit no exceptions. In few explanations of individual happenings are all the relevant prior or simultaneous happenings stated : it is assumed that the happening to be explained occurs within a set of conditions, and that only significant prior or simultaneous changes in those conditions need be mentioned. (Cf. Mr. Kneale's *Probability and Induction*, pp. 61-62 ; the essential points had already been explained by Collingwood in *An Essay on Metaphysics* (Oxford, 1940), pp. 296-312, where J. S. Mill, *A System of Logic*, Book III, ch. 5, sect. 3, is acknowledged.) The second kind of explanation is of general laws. Explanations of this kind consist solely of general laws other than those which are explained. In both kinds of explanation, and especially in the second, the logical derivation of what is explained from its explanation may require dexterity and even deep insight.

While the Hempelian theory asserts that every scientific explanation is of one of these two kinds, it distinguishes between theoretical and historical sciences. Professor Nagel has drawn the distinction with exceptional clarity. 'A theoretical science like physics seeks to establish both general and singular statements, and in the process of doing so physicists will employ previously established statements of both types. Historians, on the other hand, aim to assert warranted singular statements about the occurrence and interrelations of specific actions ; and though this task can be achieved only by assuming and using

general laws, historians do not regard it as part of their task to *establish* such laws. The distinction between history and theoretical science is thus somewhat analogous to the difference between medical diagnosis and physiology, or between geology and physics. A geologist seeks to ascertain, for example, the sequential order of geologic formations, and he is able to do so by applying various physical laws to the materials he encounters; it is not the geologist's task, *qua* geologist, to establish the laws of mechanics or of radioactive disintegration that he may employ<sup>1</sup> (FB, p. 689). There is no difference in form between historical and geological explanations, so that human history is a branch of natural history. Explanations in human history, as in other branches of natural history, exemplify the first of the two Hempelian kinds; further explanation of the laws they employ is left to the theoretical sciences, which employ explanations of both kinds. Which theoretical sciences explain the laws historians employ? Hempel himself has observed that 'those universal hypotheses to which historians explicitly or tacitly refer in offering explanations . . . are taken from *various* fields of scientific research, in so far as they are not pre-scientific generalizations of everyday experiences' (GLH, p. 470); but he would not, I think, dissent from the received opinion that the fledgeling social sciences already provide many of them, and will, as they develop, supply historians with well-confirmed laws to replace the pre-scientific generalizations on which they must now too often rely.

So far, we have simply rehearsed a theory. What reasons can be offered to prove it? Its advocates do not offer much by way of argument, preferring to elicit it by a sort of intuitive induction from specimens in natural science and natural history. Possibly they think that, once expounded, its truth is obvious; and, certainly, two lines of proof come readily to mind. They are as follows: (1) If it is supposed that an explanation need not logically entail what it explains, but may be consistent with several other possibilities, then it will fail to explain why one or other of those possibilities was not realized, *i.e.* it will fail to explain why what it purports to explain should have happened rather than something else. Now, it may be contended, no statement that a certain individual happening took place can be logically derived from other statements about individual happenings except by way of general laws. (2) As Hempel urges against a view of Mr. Mandelbaum, 'Every "causal explanation" is an

<sup>1</sup> "The Logic of Historical Analysis", *The Scientific Monthly*, vol. lxxiv (1952).

"explanation by scientific laws"; for in no way other than by reference to empirical laws can the assertion of a causal connection between certain events be scientifically substantiated' (GLH, p. 461). In other words, unless historical explanations satisfy the Hempelian theory, they cannot be verified, and should be condemned as subjective.

That both these lines of proof are *a priori* is not to their discredit. But are they valid? Although no fact can overthrow a valid *a priori* argument, in trying to decide whether or not a given *a priori* argument is valid, it is permissible to take facts into consideration. The facts I consider concern what historians actually recognize as explanations. Those who have adopted the Hempelian theory have not ignored historical explanations, but they have placed little weight on the claims historians make for them.

Let us begin with an example invented by Hempel (GLH, p. 464): 'the Dust Bowl farmers migrate to California "because" continual drought and sandstorms render their existence increasingly precarious, and because California seems to them to offer so much better living conditions'. On the face of it, this explains a migration of Dust-Bowl farmers to California by two statements of individual happenings: (1) that drought and sandstorms rendered their existence increasingly precarious; and (2) that they believed California to offer them better living conditions. The statement of the fact to be explained, however, certainly does not logically follow from these two statements; so that, according to the Hempelian theory, this explanation must, if sound, be elliptical; for what is to be explained must logically follow from its explanation, and statements about individual happenings can be interconnected only by assuming and using general laws (*cf.* Nagel, *loc. cit.*, FB, p. 689). 'Such terms as "hence", "therefore", "consequently", "because", "naturally", "obviously", etc., are often indicative of the tacit presupposition of some general law: they are used to tie up the initial conditions with the event to be explained; but that the latter was "naturally" to be expected as "a consequence" of the stated conditions follows only if suitable general laws are presupposed' (GLH, p. 464). 'Resuscitation of the assumptions buried under the gravestones "hence", "therefore", "because", and the like' (GLH, p. 466) is an exercise familiar to those drilled in syllogistic logic; by its means Hempel is able to resuscitate from his example the assumption of 'some such universal hypothesis as that populations will tend to migrate to regions which offer better living conditions' (GLH, p. 464). The resuscitation of this buried assumption unfortunately exposes a friendless

corps: 'it would obviously be difficult accurately to state this hypothesis in the form of a general law which is reasonably well confirmed by all the relevant evidence available' (GLH, p. 464). Such difficulties, it is acknowledged, arise often.

In the light of these facts, only two verdicts on historical explanation are consistent with the Hempelian theory. One, which few swallow, is that most historical explanations are simply false. The other, preferred by Hempel himself, is that they are imperfect: they are not intended as explanations proper but as 'explanation sketches'; 'such a sketch consists of a more or less vague indication of the laws and initial conditions considered as relevant, and it needs "filling-out" in order to turn into a full-fledged explanation. This filling-out requires further empirical research, for which the sketch suggests the direction' (GLH, p. 465). Since 'concrete research may tend to confirm or infirm these indications', explanation sketches 'have some empirical import' and are consequently testable (GLH, p. 466). In Hempel's example, the sketch would presumably be filled out by finding a less vulnerable universal hypothesis than that populations will tend to migrate to regions which offer better living conditions, and a more precise definition of the relevant initial conditions; if the sketch were to be confirmed both the fuller-fledged hypothesis and the more precisely defined initial conditions would have to do with attempts to better living conditions. It is for the social sciences to establish such hypotheses. The difficulties confronting this programme are formidable and, since they are not unknown, need not be dwelt on. One which is crucial is that, since in social science 'single factors cannot be easily isolated and independently measured', there cannot be any 'composition of forces', so that a complex system of laws is out of the question. If laws in social science assert only tendencies, then their operation is indeterminate: 'conflicting schools or parties can begin with the assertion of opposite tendencies and never really join in a definite issue'. (Cf. Morris R. Cohen, *Reason and Nature*, Book III, ch. 1, sect. 3(d), reprinted FB, pp. 672-673.) Mr. Patrick Gardiner has proposed a further refinement, partly designed to obviate difficulties of this kind. Such hypotheses as that populations tend to migrate to regions which offer better living conditions are open or porous in texture: thus, Hempel's example does not determine how much better the living conditions must be, or which living conditions are significant; 'any attempt to demarcate precisely the area of its application' is precluded (*The Nature of Historical Explanation*, pp. 93-94). So much might appear to be implicit in Hempel's

exposition, but Gardiner goes further. 'The historian, like the general or the statesman, tends to *assess* rather than to *conclude*. . . . There is, indeed, a point in terming (for example) the explanations provided by the historian "judgements". . . . These assessments or judgements, however, are not 'made, or accepted, in default of anything "better": we should rather insist that their formulation represents the *end* of historical inquiry, not that they are stages on the journey towards that end' (*ibid.* pp. 95-96). Gardiner cannot have it both ways: if he denies that an historical explanation sketch merely shows the way to something better, he must renounce Hempel's method of confirmation, which is to discover something better in the direction towards which it points. If he knows of any other method of confirmation, Gardiner does not vouchsafe it;<sup>1</sup> the only one he mentions, that of practical success, which may be employed in judging the assessments of generals or statesmen, plainly does not apply to those of historians. So, despite his disclaimer (*ibid.* p. 95), if historical explanations are merely assessments or judgements, then they are "subjective" in a vicious sense.'

Those advocates of the Hempelian theory who are not eager to deny history all scientific value, are fast in a dilemma: either historical explanations presuppose false universal hypotheses, or they are mere sketches which it is extremely difficult to supersede with anything better, or which have degenerated into subjective assessments. The best face Nagel can put upon the situation is to claim that, 'although there are often legitimate grounds for doubt concerning the validity of specific causal imputations in history, there appears to be no reason for converting such doubt into wholesale skepticism' (FB, p. 700). There is, besides, an excuse: 'the probable cost of remedial measures in terms of labor and money seems staggering' (*ibid.*).

Do historians accept either this depreciation or this cold comfort? How do they view their own efforts at explanation?

There can be no general answers to these questions. Many historical explanations are bad, since bad historians we have always with us. And philosophers are sometimes obtuse in selecting or fabricating examples. Thus Gardiner devotes

<sup>1</sup> In the pages which follow (pp. 96-99), Gardiner appears to suggest that historians "fill in", and so justify or support their explanations, by elaborating detail. It is hard to see how providing details of the hard lot of Dust-Bowl farmers, and of their dreams of Californian opulence, could scientifically support Hempel's explanation of their migration, though it might persuade by inducing an historically vicious use of "empathy".

two pages of his book (pp. 96-97) to describing what an historian would do if faced with the question: 'Why did Louis XIV die unpopular?'. An historian could not be faced with this question unless he could prove the fact; and it is hard to see how he could prove *this* fact unless he also knew its explanation (*cf.* R. G. Collingwood, *The Idea of History*, p. 214). By contrast, Hempel's factitious example is a happy one; and many good historical explanations resemble it. Thus Mr. J. A. Williamson, in his excellent short history, *The Evolution of England*, explains the Scandinavian invasion of Britain in the first half of the ninth century thus: 'The Norsemen and Danes who sailed south to the Irish Sea and to the shores of the English Channel were plunderers first and settlers by an afterthought. Like the early Anglo-Saxons, they came to sack a civilised land, and only when they had stolen all they could get did they think of occupying its soil'<sup>1</sup> (pp. 47-48). If any buried assumption is to be resuscitated from this, it would be, I suppose, that all men, or all Norsemen and Danes, and perhaps Anglo-Saxons too, are plunderers first and settlers by afterthought. Plausible this assumption may be, but it would be fantastic to suppose that Williamson would consider his explanation weakened if exceptions could be found to it. Mr. J. N. L. Myres' explanation of the Saxon invasions in the fifth and sixth centuries (vol. i. of the *Oxford History of England*, pp. 339-351), though too long to quote, also resembles Hempel's example. Now, it is plain that both Williamson and Myres consider their explanations true, and both cite evidence which is convincing to an amateur. It is equally clear that they do not think them imperfect, or mere sketches. They are not final, because further questions must be asked. But to the question, Why did those groups of Norsemen and Danes sail south? Williamson professes to have given an historically perfect, though corrigible, answer. Myres implicitly makes a similar claim.

The Hempelian theory, therefore, contradicts at least some good historians' opinions about their explanations. Before considering the implications of this conflict, we must consider a way of amending the theory to bring it into accord with these opinions. If historical explanations should rest, not on laws, but on approximations to them—on generalizations that are mostly true—then, it has been argued, they would not be refutable by inconvenient exceptions. This amendment must be distinguished from Gardiner's interpretation, which is that some historical explanations rest

<sup>1</sup> *Cf.* F. M. Stenton, *Anglo-Saxon England* (vol. ii of the *Oxford History of England*), 2nd edn., pp. 240-244.



on laws which are genuinely universal, but of which the area of application is open and indeterminate. One objection seems to be decisive: an explanation which rests on an approximate generalization cannot entail what it explains, and so must fall short of the *a priori* condition that it may allow no alternative to what it explains.

## (2) *Non-Hempelien Historical Explanations*

That historical explanations of the kind exemplified in the specimens we have given belong to a large class of non-Hempelien explanations has been recognized by Gardiner in Part IV of *The Nature of Historical Explanation*, and by Mr. J. W. N. Watkins in his essay, "Ideal Types and Historical Explanation" (*British Journal for the Philosophy of Science*, vol. III, 1952, reprinted in FB). Both Gardiner and Watkins have derived from the work of Professor Ryle an analysis which, in the main, mine will follow. My justification for traversing once more this well-surveyed territory is to show how different it looks if one goes in a sceptical spirit, without the preconception that every happening must have an Hempelian explanation.

The Hempelian theory, I have argued, may be defended upon two grounds: that Hempelian explanations alone logically entail what they explain, and that they alone are verifiable by producible evidence. If there are non-Hempelien explanations, it is imperative to answer three questions. Are they intended to satisfy the conditions on which the Hempelian theory rests, and if so, at what point are the arguments for the Hempelian theory defective? Are the conditions they are intended to satisfy scientifically defensible? Are they ever satisfied?

There is no doubt that our specimens are intended to be verifiable by producible evidence; that at least is shown in Collingwood's discussion of historical evidence (*The Idea of History*, part v, sect. 3, esp. pp. 256-261). What of the first condition, that an explanation must logically entail what it explains? With some diffidence, I have concluded that, despite apparent exceptions, historical explanations are intended to satisfy this condition also.<sup>1</sup>

<sup>1</sup> Mr. Dray has distinguished between explanations of why a thing happened and explanations of how it could have happened (*Phil. Quart.* iv (1954), 19-20), and has shown that sometimes historians seek not the former but the latter (*ibid.* pp. 24-27). Although the Hempelian theory must be extended to allow for the latter, Dray would perhaps agree that most historical explanations, and the most important of them, explain why things happened; the Hempelian theory of these is undamaged by his results.



Some explanations are incomplete: they do not entail what they explain unless something is added to them. Hempel's factitious example is incomplete in this sense. To complete it, it would be necessary to show that the Dust-Bowl migrants thought that California alone offered them better prospects, or that the prospects there were significantly better than elsewhere. Similarly, Williamson's explanation, as it stands, incompletely explains why the Danes sailed into the English Channel and Irish Sea rather than, say, to the Mediterranean; on the other hand, as an explanation of the character of the invasion, *e.g.* of the fact that the invaders did not at first occupy the land, it is complete though not final. A final explanation is one which leaves room for no further demand for explanation: it is self-explanatory within the presuppositions of the inquiry within which it is given. Such explanations are usually sought only in the exact sciences, *e.g.* mathematics. An explanation may be complete, in the sense of requiring no addition in order to entail what it explains, and yet not be final: thus an Hempelian explanation of a planet's position at a certain time by deriving it, according to Kepler's laws, from information about its positions with respect to the sun at several prior times, would be complete, but not final; for explanations could be sought both of its prior positions and of Kepler's laws.

It is often superfluous to complete historical explanations, because the completion would be obvious, or easily ascertained, or boring. Besides, there are numerous explanations which it is impossible to complete for lack of evidence: these are defective, and admitted to be so by historians. Neither of these kinds of incompleteness poses any theoretical problem. There are, however, explanations which do not entail what they are said to explain, but which it appears to be in principle impossible to complete. If this appearance is not dissipated by analysis, then an exception must be admitted to our account of the conditions historical explanations are intended to satisfy. If a literary historian should explain Dr. Johnson's famous letter to Lord Chesterfield as a deliberate expression of his just indignation, it would be natural to concede that this is an explanation, whether true or not, but an incomplete one. Dr. Johnson might have expressed his indignation in other ways: by a satirical essay rather than a letter; in verse rather than in prose; or, at least, in words other than those used in his letter. Clearly then, the explanation does not entail that Dr. Johnson wrote the letter he did write and not something else. Yet could this explanation, even in principle, be completed? Certainly Dr.

Johnson may have had reasons for writing a letter and not an essay, in prose and not in verse, and for choosing certain epithets and not others : given evidence, literary historians can pursue such questions surprisingly far. But no reader in his right mind would demand an explanation why the whole letter was written just as it was ; nor would any competent historian seek to furnish it. I venture the following solution. The explanation given of Dr. Johnson's letter does *not* incompletely explain why it was written just as it was, but completely explains why it has certain general features. I think this becomes clear if we consider what would contradict it : it would be other apparently incomplete explanations, such as that he bore a grudge, was advertising himself, or was vindicating his wounded pride. From each of these, as from the explanation they contradict, it is possible to deduce that Dr. Johnson's letter has certain general features. The question of which explanation is right may be settled by considering whether the letter has the general features required by one rather than those required by the others.

If historical explanations, in as much as they are complete, are intended to satisfy both the *a priori* conditions on which the Hempelian theory rests, must they not satisfy the Hempelian theory ? Not unless that theory in fact rests on its foundations, and it does so only if it is true that a statement about one individual happening cannot be logically derived from statements about other individual happenings except by the mediation of general laws. The clearest and most important of recent demonstrations that this crucial proposition is false has been provided by Ryle. Although the significance of Ryle's work for the philosophy of history has been recognized, notably by Gardiner and by J. W. N. Watkins, not all its implications have been understood. I dare not hope that what follows may exhaust them.

The cardinal point of Ryle's analysis is that ordinary physical explanations are often not Hempelian (*The Concept of Mind* (London, 1949),<sup>1</sup> pp. 88-89, 123-125). A man may explain the breaking of his windows by mentioning a cause, 'They were stoned' ; or he may explain why his windows broke when stoned by asserting that they were brittle (CM, p. 88). The first explanation corresponds to part of an Hempelian explanation—the statement of significant prior or simultaneous happenings. The second explanation differs from anything recognized in the Hempelian theory, which presupposes that the only way of

<sup>1</sup> Hereafter referred to as CM, followed by a page number where necessary.

deriving the statement that certain windows broke from the statement that they were stoned is by the allegedly buried general law, 'All windows break when stoned'. A general law is a hypothetical proposition which is general in that it mentions no individual. Taking 'F' and 'G' as predicate variables, a general law might have the form, 'If anything is F it is G', though most laws are more complex in form than that. If we use the word 'open' as a technical term meaning 'containing no mention of individual things, happenings and so on', and 'closed' as meaning 'containing some mention of individuals', then laws are completely open (CM, p. 123). What of the statement, 'Those windows were brittle'? There is no doubt that to make that statement is to assert a general hypothetical proposition about those windows, which is, roughly, that if sharply struck or twisted they would not dissolve or stretch or evaporate but fly into fragments (CM, p. 89). In some respects this hypothetical is open: it does not mention any individual thing that may strike them or any individual occurrence in which they were struck, or in which they flew into fragments. But in one respect it is closed: it mentions certain individual windows. Its form is not 'If anything is F it is G', but 'If these individuals should be F they would be G'. There are degrees of openness and closedness. Instead of saying 'Those windows are brittle', you might say 'Those windows would fly into fragments if hit by this stone thrown with the force of a ten-year-old', or even 'Those windows would break if Puck were to touch them with Oberon's sceptre at midnight next All Hallows' Eve'; the second of these is less open than the first, and the third than the second. To hypotheticals which are partly closed Ryle has given the name 'law-like statements': they resemble general laws in being general hypotheticals; they differ in not being completely open (CM, pp. 123-124).

Whether or not this purely abstract analysis is relevant to history depends on Ryle's substantive doctrines of mind. Historians often explain the actions of men by referring to their plans, schemes, and intentions; and these they often explain by referring to the ends it is thought their execution will achieve, to the means thought permissible or not in achieving them, or to the enjoyment found in carrying out one rather than another. The adoption of ends is sometimes itself explicable by referring to reasons thought sufficient, and to scientific, ethical or religious principles. Within limits, some things a man does, or even believes, may be explained by referring to his character. These limits, however, are important: just as you cannot determine

the degree to which a manuscript of an ancient Greek poet has been depraved except by deciding whether its individual lections are good or bad, so you cannot ascertain a man's character, itself a thing which changes, except by considering his capacities, the uses to which he puts them, the ends he pursues, and the means he thinks permissible to attain them. Now Ryle has contended that explanations of all these kinds resemble the explanation that certain windows broke when they were stoned because they were brittle: in giving them historians are asserting law-like propositions about the publicly observable, and, to a lesser degree, the private and unobservable states and activities of the pertinent historical agents.

To revert to our earlier example, Williamson's statement that the Danes who invaded England in the early ninth century were plunderers first and settlers by afterthought. This is a statement about the ends adopted by those Danes, and Williamson uses it in several explanations. Although its texture is highly 'porous' (to use Dr. Waismann's valuable expression), it immediately implies the law-like statement that if those Danes had opportunities of sufficient plunder in a territory, they would not settle in it; and, with other statements, it implies a host of further law-like statements, such as that if they produced or transmitted a literature it would glorify war rather than farming, and that if they had a religion it would permit them to live as warriors. These and similar statements unfold the meaning of 'They were plunderers first', and their law-like character makes it is possible to derive, *e.g.* the fact that the Danes did not settle for a generation, from the fact that, during that period, Anglo-Saxon England provided opportunities for sufficient plunder.

Even among those who understand them, the principal theses of *The Concept of Mind* have not been universally accepted. I have no scope for a formal defence, and indeed, do not think them equally defensible. Those relevant to history concern the various things which Collingwood has classed together under his compendious notion of 'thought', which appears to include everything mental except the flow of immediate consciousness, and which Ryle discusses under the traditional distinctions of thought, will, and certain types of emotion. The general view Ryle takes of these matters is independent of his conclusions about some feelings, images, and some sensations and sense-perceptions—indeed of the greater part of his theories of perception and imagination; and the arguments in its favour appear to me overwhelming. The view to which it is opposed is that Williamson's statement about the ends the Danish invaders had

in view asserts that they performed, continuously or intermittently through the relevant period, private introspectible mental acts of a certain kind; and that performing acts of that kind is what is meant by 'having an end in view'. Except for negative arguments against alternatives, the only evidence I know for this position is that often a man knows without reflection what ends he has in view. This, however, does not entail that having an end is performing an introspectible mental act, unless either the conclusion is made trivial, *e.g.* by making having an end a paradigm of a mental act, or the premise is conjoined with another and false one, that all a man can know of his mind without reflection is that he is performing a private introspectible act or experiencing a private introspectible state. Furthermore, the supposititious acts and states appear to have no function in ordinary or scientific thinking about human action. 'Here I should like to say: a wheel that can be turned though nothing else moves with it, is not part of the mechanism' (Wittgenstein, *Phil. Inv.*, § 271). Ryle's adversaries themselves concede that a statement which, on their view, asserts that an introspectible mental act has occurred, will entail the law-like propositions in which Ryle would find most of their meaning; but familiar anti-reductionist arguments seem not to apply. If an historian knows to be true the pertinent law-like propositions about the conduct of the Danes, does he need also to know about their introspectible acts and states in order to pronounce on their ends and intentions? Could any information about these introspectible acts and states affect his verdict? (*Cf.* CM, pp. 65-69). '... Theories which made no mention of the deliverances of "inner perception" were at first likened to "Hamlet" without the Prince of Denmark. But the extruded hero soon came to seem so bloodless and spineless a being that even opponents of these theories began to feel shy of imposing heavy theoretical burdens on his spectral shoulders' (CM, p. 328).

We are now in a position to answer the first of the three questions with which this part of our inquiry began. Historical explanations of the kind exemplified by our specimens are intended to satisfy the conditions on which the Hempelian theory is founded: they must both admit of scientific substantiation, and logically entail what they explain. However, and here the Hempelian theory is defective, they may fulfil these conditions without presupposing any general laws. Our specimens presuppose only law-like statements, which, as Ryle has shown, suffice to connect one happening with another.

To the second question, 'Are these conditions scientifically

defensible ? ' a ready answer would be that they must be, provided only that Ryle's analyses hold, and that the fundamental Hempelian conditions themselves be defensible. There is, however, one objection which must be answered. ' The suggestion has been made that [law-like] <sup>1</sup> statements while not themselves laws, are deductions from laws, so that we have to learn some perhaps crude and vague laws before we can assert them ' (CM, p. 124). That is to say, law-like statements directly presuppose laws. If that were so, the criticism of the Hempelian theory which I have so painfully elaborated would be idle. Although this objection is widely received, I know of no reason why it should be ; and Ryle's brief confutation seems to be final. ' In general, the learning process goes the other way. We learn to make a number of [law-like] statements about individuals before we learn laws stating general correlations between such statements ' (CM, p. 124). A man may know that his windows are brittle, but not know the laws from which, together with information about their composition and size, his law-like knowledge may be deduced. A more ultimate objection may now be met : even if law-like statements do not presuppose laws from which, together with other information, they could be deduced, nevertheless it may be in principle true that there must be such laws, even though they be unknown. That this is so is the doctrine of determinism, to which we shall return. At present it suffices that the question whether our specimens are legitimate explanations is unaffected by whether or not the law-like statements they employ may be derived from laws as yet unknown. Without presupposing general laws, they entail what they explain, and may be known to be true : so that the conditions they satisfy, while differing in one respect from those required by the Hempelian theory, are yet scientifically irreproachable.

Finally, Do any historical explanations satisfy these conditions ? The greater part of the preceding investigation has been devoted to the logical relations between explanations and what they explain, since we received as conclusive Collingwood's demonstration that historical explanations are put forward as capable of scientific substantiation. Well, how can they be scientifically substantiated ? Historians' procedure is similar to that employed for explanations involving laws : they deduce from the explanation alone, or from it together with other propositions, that certain evidence should be found ; if they do not find it, they reject or revise either the explanation or some

<sup>1</sup> I have substituted 'law-like' for 'dispositional' here and in the sequel.

of the other propositions; if they do, they accept the explanation unless some alternative also explains the evidence; if there are alternatives, they decide between them by the same procedure. Consider again Williamson's explanation of the course of the Danish invasion. One may deduce from it that Saxon records will show that the communities defeated by the Danes in battle were plundered and abandoned, and that the resources of the Saxon communities were exhausted by these raids before their occupation by the Danes; unfortunately little archaeological evidence is available (F.M. Stenton, *Anglo-Saxon England*, p. 696). If reliable evidence of this kind is found (and in order to use an historical record, one must be able to demonstrate in what respects it is reliable), unless another explanation is consistent with it, Williamson would be entitled to claim that his explanation is proved.

Since a thorough investigation of the process of historical reconstruction, or hypothesis, and proof may be found in the second and third Epilegomena to Collingwood's *The Idea of History*, I shall dwell only on the most striking difference between the proof of an historical explanation of the kind to which our specimens belong, and that of an Hempelian explanation. It is this. The general laws on which Hempelian explanations turn, being open, apply to all times and places, so that it is usually possible to test them either by contriving artificial experiments, or, as in astronomy, by observing recurrences in the present. By contrast, the law-like propositions on which our historical explanations turn, being partly closed, apply only to mentioned individuals, so that the only evidence for or against them is in what survives of the traces, effects, and influence of those individuals' actions: to test them either by experiment or by observing recurrences, is not something lamentably difficult; it is strictly inconceivable. This fact has advantages and disadvantages. The quantity of evidence relevant to many historical law-like propositions is so exiguous that they cannot be scientifically established or refuted: here natural history, the explanations of which are Hempelian, has an unquestionable advantage. On the other hand, as Collingwood pointed out, it follows that our historical explanations are not 'permissive'; that is, they are not affected by finding exceptions to any hypothesized general law (*The Idea of History*, p. 261).

### (3) Concluding Remarks

Since it is impossible to anticipate the innumerable objections which might be advanced against the conclusions I have reached,



I have chosen at random a few questions for discussion, each in itself important. (i) Do our conclusions apply to all or most historical explanations, or to a few only? (ii) Since historians evidently do employ generalizations, how do they if not in explanations? (iii) Since it is said to be philosophically demonstrable that every happening is explicable in principle according to general laws, are not historical explanations acceptable only *faute de mieux*, to be replaced if possible by others of the same type as those of natural history?

(i) Since, of necessity, I have worked from selected examples, it should be superfluous to disclaim comprehensiveness. My selection, however, was not capricious: neither economic, nor geographical, nor possible sociological explanations were overlooked. It is a fundamental error to conceive such explanations as of types different from those we have examined. Both Hempel's explanation of the migration from the Dust Bowl and Williamson's of the Danish invasion of England are, after all, economic; and I have borne in mind throughout two other specimens which I have recently had occasion to study, Charles A. Beard's celebrated *An Economic Interpretation of the Constitution of the United States* (1913), and Professor Bielenstein's explanation of the fall of the Emperor Wang Mang and the restoration of the Han dynasty by 'a change in the course of the Yellow River, a natural disaster which could not have been prevented and therefore was no one's fault', but 'whose consequences affected half of the population' (*The Restoration of the Han Dynasty*, Museum of Far Eastern Antiquities, Bulletin No. 26 (Stockholm, 1954), p. 154). In these works, as in all genuine historiography, economic or geographical factors are exhibited as operating by way of the distinctive responses<sup>1</sup> different historical agents make to them; and those responses are unintelligible except in terms of the knowledge, efficiency, character and way of life of whoever makes them. If sociology should develop as economics has, the same point will hold of sociological explanations. An historian, who has reason to presume that his readers are familiar with the kind of response his characters would make to certain situations, may

<sup>1</sup> The word 'response' is stolen from Professor A. J. Toynbee's *A Study of History* (London, 1934), vols. i-ii, though I cannot accept Toynbee's presupposition, that if the geneses of civilizations are not the result of biological factors or of geographical environment acting separately, they must be the result of some kind of interaction between them. Professor Geyl's judgement is sound, that although 'the striking formula of challenge and response' is 'a find', it is a mistake to follow Toynbee in formulating laws in terms of it. 'There is no question here of a law. . . .' Cf. *Journal of the History of Ideas*, ix (1948), 99.



properly mention only the relevant situation in explaining their conduct: should he do so, it would be a howler to overlook what he has presumed, and profess to resuscitate from his explanation some preposterous universal hypothesis.

When, in the concluding chapter of his *General Theory of Employment, Interest and Money*, Lord Keynes referred to 'the economic causes of war, namely, the pressure of population and the competitive struggle for markets' (p. 381), he implied no such absurdity as that competition between Norway and Switzerland in the search for markets is a cause of war; and in mentioning pressure of population he had in mind, not nineteenth century Ireland or early twentieth century India, but, primarily, Mussolini's Italy and modern Japan. He presupposed no universal hypothesis of any kind.

A more recent socio-economic example is found in a correspondence in *History Today*, v (1955), 554-555. To Mr. H. R. Trevor-Roper's explanation of the later phase of the Great Rebellion—the so-called 'Independent' revolution—as a revolution of the lesser and declining gentry aimed at anti-capitalist decentralization, a correspondent objected, 'It is surely curious that a revolt of the poverty-stricken gentry against Court and City should receive least support from those areas that were furthest away from both and most poverty-stricken'. So it would be if Trevor-Roper's explanation had been Hempelian. That it was not is unmistakable from his reply. '... We must remember that not all members of the same social class respond to the same pressures by the same social attitude. Some members of a depressed class may rebel, others submit and become defeatist: the difference is in their personal character and their political organization.' Provided that the rebellious Independents were of the class to which Trevor-Roper assigns them, and acted from the motives he reconstructs, his explanation will stand whatever may be true of the loyal lesser gentry. In some cases at least nothing comes of pushing the matter any further. Kentish Sir Byng stood for his King; his neighbour, Kentish Sir John, became an Independent rebel; the difference was in 'their personal character'.

It may be objected that this does not meet the point of most importance. To cite another example from Williamson: 'The transmission of the precious metals in bulk from across the ocean began with the capture of Mexican gold by Cortes in 1519, continued with the looting of Peru by Pizarro in 1532, and settled down into a regular stream with the working of the silver mines in both regions a few years later. This influx of gold and

silver spread all over Europe and caused money to grow less valuable, or, in other words, prices to rise' (*The Evolution of England*, p. 213). This explanation presupposes both that in the early sixteenth century an economic system obtained in Europe, from the laws of which it could be deduced that if there should be a great influx of gold and silver, unaccompanied by any compensating occurrence, prices would rise, and that at that time there was no compensating occurrence. In short, it really is Hempelian. It explains, however, the conditions of historical action, *i.e.* a rise in prices, not action itself, *i.e.* what response was made to that rise. To this it might be objected that a rise in prices is a matter of many acts by many persons: that is true, but beside the point; for the explanation presupposes that a certain economic system obtained, *i.e.* that the persons concerned would, in certain circumstances, act in certain ways. How is that explained? If it is only explicable in the way I have described, as a response proceeding from the knowledge and character of the agents concerned, then the Hempelian explanation rests on a non-Hempelian one.

There are, however, economic determinists, who maintain both that a thorough understanding of any economic system will include knowledge of the laws according to which any instance of that system must develop into an instance of a different system, and that the economic system of a society determines the general character of its political, religious and artistic life. These are, roughly, doctrines of Marx and Engels: if they were true, our analysis would at best hold of history as it is, not of history as it should be. I think both doctrines to be false, though I cannot here attempt to refute them. I refer, however, to a line of investigation pursued by Mr. Leonard Krieger in his essay, "Marx and Engels as Historians" (*Journal of the History of Ideas*, vol. xiv, 1953). Krieger shows that Marx and Engels found the concepts of class their earlier historical work seemed to require, especially that for *Class Struggles in France*, to diverge from those of their political and economic theories; and argues that this led them to distinguish class as an economic phenomenon from class as something of which its members are conscious, and to explain the latter by a theory of false consciousness. They concluded that history, as written, is the history of false consciousness, and that the true key to understanding historical events is not history, but 'dialectic'. There is undoubtedly some truth in the theory of false consciousness; but historians, while finding that a certain group had a false view of its situation, would nevertheless correctly explain the actions of its members in terms

of it. The main contribution of Marx to history was his enlargement of other historians' insight, not his circumvention of history by dialectic.

(ii) Do historians employ generalizations, and, if so, how? We have already remarked that historians sometimes explain conditions of historical action in the Hempelian way; where, as in Williamson's explanation of price inflation in the sixteenth century, such explanations themselves involve action, we have suggested that they derive from non-Hempelien explanations. Historians therefore employ universal hypotheses at least to the extent required by these explanations, and universal hypotheses are often thought of (misleadingly) as generalizations. More importantly, historians employ generalizations to guide their investigations. Professor R. M. Crawford, illustrating another point, furnishes this excellent example. 'From a rough, unsystematic, unformulated generalization of experience, we predict what we would expect to find in the matter to be explained. For example, in preparing a lecture on the Port Phillip Separation Movement, I felt that to account for six years of organized and sustained agitation, I should expect to find that those who organized and shared in that agitation were injured by their connexion with New South Wales in some important and probably material fashion. And of course I soon found that they were . . . ' (*Historical Studies, Australia and New Zealand*, iii (1944-49), p. 165). The utility of the generalization, 'Those who organize and share in a sustained agitation against a political arrangement must be injured by that arrangement in some important and probably material fashion', does *not* depend on its universal truth. Its function is to direct historians to look for something, not to assure them of finding it. To find that, in an individual case, say, the agitation against partition in Ireland, or that for Enosis in Cyprus, the generalization does not hold, would entail neither that the case was inexplicable nor that the generalization should be revised. None of this contradicts our thesis, which is not that historians do not use generalizations, nor even that they do not use them in any of their explanations, but that they do not use them in explaining historical actions.

(iii) Ought historical explanations to be displaced as soon as possible by others of the same type as those of natural history? This question may arise from more than one set of presuppositions. One such set is sometimes professed under the engaging name of "Scientific Humanism". Its creed is that all science is one; that scientific explanations all presuppose general laws; and that every happening has a scientific explanation, if not in fact then

in principle. "Scientific Humanism" may be defended on diverse grounds: by metaphysical proofs showing that unless it were accepted, discourse would be impossible; by arguments about the direction in which science is progressing; by pragmatic considerations; by deduction from a religious faith like Deism. It may be assailed on grounds as various. Since such considerations, although proper, are beyond my present scope, I must be content to record a few pertinent facts: (1) explanations of historical actions are, in our present state of knowledge, independent of general laws; (2) many such explanations are verifiable and verified; (3) their subsumption under general laws, though possible *a priori*, is at present purely visionary; (4) the social sciences have not established any genuine laws, and much of what they have accomplished has been fruit of the historical method. These facts do not refute "Scientific Humanism", but they may discourage us from persevering with it.

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## II.—PLATO ON STATEMENT AND TRUTH-VALUE

BY JASON XENAKIS

### I

PLATO discusses the notions of false, true and statement in a number of places, but *Sophist* 261e-3b stands out. I propose to analyse, and not merely to reproduce in other words, this passage because I expect to make it evident that it has been unduly if not regretfully neglected by those who concern themselves with such matters. I am almost tempted to retrodict, for example, that the Theory of Descriptions would not have been born had this passage been paid the attention it deserves. In any case, 'the present king of France is bald' would not have perplexed anybody because it would not have been even seriously considered, let alone chosen as a legitimate specimen of a false statement, or indeed of a statement.

Further, if my analysis is correct, it will follow that the passage has been misinterpreted, among other specialists by Cornford (*Pl.'s Th. of Kn.* pp. 303-317) whose interpretation, or type of interpretation, is the most influential, if it is not generally taken for granted. Cornford holds that Plato has in mind here, as in the *Theaetetus*, the Forms; that the Forms, like the *koina*, are meanings; that one must invoke some sort of one-to-one correspondence analysis of truth-value involving the previous two theses in one's interpretation or understanding of Plato's account of truth-value; and that only thus can Plato meet 'the Sophist', and only thus can the alleged 'vagueness', 'ambiguity', 'obscurity' and 'difficulty' of the passage be explained.

But first, there is no trace of the Forms or of its Theory in our passage. And it is not self-evident, as Cornford seems to suppose, that every time Plato wants to solve or dissolve a puzzle he has the Theory of Forms at the front or back of his mind, or that the Theory fulfilled a cure-all function in his thought—that it assumed the role of a passkey to all problems. Cornford seems to feel surer about that Theory than its own originator. He seems to forget that in the *Phaedo* (100-101) it is treated as a hypothesis not as a dogma; that in the tenth book of the *Republic* it is

introduced as an assumption or postulate (*tithesthai*, 596a7); that in the *Sophist* (248a) it is ascribed to others—'the friends of the Forms'—probably some youngsters in the Academy; and that in the *Parmenides* (130b ff.) it is just about reduced to an absurdity. (It is worth adding that some interpreters are of the opinion that none of the objections to the Theory in the *Parmenides* is valid: see, e.g. Peck *C.Q.* 1953. Plato, of course, does not conclude the discussion (135) by rejecting some form or other of the Theory despite the apparently devastating criticism of it.)

Next, Plato nowhere says that the Forms—or the *koina*—are meanings, and fortunately so. Nor does he hold that a falsehood is meaningful because its predicate 'stands for' a Form which the thing the statement is about fails to exemplify. Cornford seems to think that unless Plato holds this he cannot 'explain' falsehood, and thus cannot meet 'the Sophist'. But had this been the case Plato would not have explained falsehood but would have produced instead a wrong theory. Cornford seems to think that a statement is meaningful if and only if its predicate 'corresponds to' or 'stands for' something-or-other. But, for one thing, the predicate *presupposes* referring. For another, a false statement is meaningful even though, indeed despite the fact that, its predicate fails to depict a property of the thing the statement's subject is about. And that is how Plato talks. I mean he says that to utter a falsehood is to present a thing as it is not. He does not say, nor does he *mean* to say: though of course I've never been in his head: it is to refer to a Form which the subject's referent fails to instantiate.

It is only when we approach our passage with different—or wrong—conceptions of truth-value that we can't make sense of it without Cornford's elaborate equipment, or that we find its doric simplicity 'vague', 'obscure', 'ambiguous', etc., as Cornford does.

Before I start dealing with the passage let me suggest that if Cornford's interpretation will have to go, then all this talk, derogatory or not, of the Platonic theory of meaning or of Platonic meanings and the like, will have to go too. For this talk is largely based on some or all of the following Cornfordian theses: that Plato originated or is responsible for the view that meanings are Forms or *vice versa*; that meanings are not glued to expressions but referred to by at least 'common names'; and that for a statement to be meaningful it and its ingredients—subject and predicate—must 'correspond to' or 'stand for' 'entities'—'facts', sense-objects, or Forms, as the case may be.

## II

*Sophist* 261e-263b, condensed, runs as follows (Cornford's and Jowett's translations modified):

STRANGER: Words which have a meaning when in sequence may be connected, but words which have no meaning when in sequence cannot [logically] be connected. [Further] a statement never consists solely of nouns spoken in succession, nor yet of verbs apart from nouns. For example, 'walks runs sleeps' and so on with all the other verbs signifying actions—you may utter them all one after another, but that does not make a statement. And again if you say 'lion stag horse' and any other nouns given to things that perform actions, such a string never makes up a statement. When one says 'man learns' [on the other hand] this is a statement of the simplest possible sort. It does not merely mention something but completes something, by weaving together verbs and nouns. Hence we say it 'says' something, not merely 'mentions' something, and in fact it is this complex that we mean by the word 'statement'. And so, just as some things fit together and some do not, so with the vocal signs: some do not fit, but those that do fit make a statement. Now whenever there is a statement, it must be about something; it cannot [logically] be about nothing. And it must [logically] be of a certain kind. Now let us fix our attention on ourselves. I will make a statement to you, then you are to tell me what the statement is about. 'Theaetetus is sitting'—not a lengthy statement. Now it is for you to say what it is about.

THEAETETUS: Clearly about me.

STR.: Now take 'Theaetetus (with whom I am now speaking) is flying'.

THE.: That [statement] too can only be said to be about me.

STR.: And we agreed that any statement must be of a certain kind.

THE.: Yes.

STR.: Then what sort of property can we assign to each of these?

THE.: One is false, the other true.

STR.: And the true one states about you things that are as they are. Whereas the false statement states about you things different from the things that are. And accordingly states things that are not [do not hold of you] as if they are [did].

Logos, as notion and word, pervades Plato's writings, as Brice Parain shows in his *Essai sur le Logos Platonicien*. (See also Cross, *MIND*, 1954.) The word, however, is very ambiguous, though in many if not most of its occurrences in Plato it has to do with language. It means: sentence, statement, discourse, language, speech, formula, definition, literary composition, reason, ground, account, proportion (cf. *Tim.* 53a8),



mark or necessary mark (for references see Parain and Cross). Plato uses it in all these senses. In our passage, however, he appears to use it in only the first two senses, switching from the second—as when he speaks of the *poion* of *logoi* and about their truth or falsehood—to the first, as when he says that you talk or say (*legein*) with *logoi* not just with names (*onomata*); though of course I cannot tell whether—or assume that?—Plato made the linguists' (de Saussure, A. H. Gardiner) distinction between Language (sentence) and Speech (statement), or even Aristotle's distinction between assertional and non-assertional *logos* (*Interpr.* 17a1-9, cf. *Poet.* 1456b10). In any case, since Plato is here intent on establishing, among other things, that falsehood is possible and thus refuting his opponents who deny this (see what precedes our passage, also *Euthyd.* 286, *Crat.* 429), he is clearly speaking of *logoi* primarily *qua* acts of speech, statements. Besides, it is improper to say that sentences are true or false, just as it is generally improper to say that the non-proper name entries in a dictionary mention or describe any particular thing. Echoes of the definition of *logos* given in our passage can be found in *Crat.* 431b-c, *Th.* 202b, *Let.* 7, 342b, 3b. *Onoma* too is ambiguous. It means word, name, proper name, noun; and Plato uses it in all these senses, though in our passage he seems to be using it in the sense of noun only, or of name including proper names, as is evident from his examples of truth-value. Also, according to some commentators *onoma* sometimes fulfils the naming- or mentioning-job, just as, by contrast, *rhēma* sometimes means predicate—'what is said of something', which, interestingly enough, is precisely how Apollodoros defines *katēgorēma* (Diogenes L., *Lives*, vii. 64). If so, 'verb', a grammatical term, is not always an appropriate rendering of Plato's use of *rhēma*. (Cf. e.g. Stenzel, *Pl.'s Meth. of Dial.* pp. 126-127.)

Now Plato seems here to be eliciting the following conditions or rules:

1. A string of words which makes no sense is logically illegitimate.

1.1. A mere collection of verbs or nouns, or of predicates or mentionings, is not *logos*. What you must have is a combination of these. (Cf. *Crat.* 425a, *Arist. Cat.* sect. 4, *Int.* sects. 1-5.) At 253a ff., Plato hints at the need for a *technē* of logical grammar, the name he gives to it being dialectic. See also 259e ff., 251 ff.

2. Mentioning or naming (*onomazein*) is not yet talking or saying (*legein*). The latter is carried out with *logoi* not just with *onomata*. The unit of discourse, in other words, is sentence not term. (Cf. *Th.* 201c ff., *Ar. Int.* 17a17-20.) *Logos* is the ter-



minus or end-product of an operation: see the occurrence of *perainei* at 262d4, which, following Professor Ryle, I translated as 'completes'.

3. A statement must be about (*peri*) something (*tinós*). If it is not about something (*metinós*) or if it is about nothing (*mēdenos*), it is not a statement. (Repeated at 263c. Cf. *Phil.* 37a.)

4. Finally, a statement must have truth-value, as we would say today: it must be 'true or false'. (Cf. *Phil.* 37bc.)

(1) and (1.1) are syntactical or formation rules. (2) brings out, among other things, the difference between talking and starting to talk, e.g. naming, calling, mentioning, identifying. (3) I shall call the subject-matter condition. And (4) anticipates Aristotle's principle of excluded middle.

Moreover, the subject-matter and truth-value requirements are two sides of the same coin: you don't have the one without the other. Or again, the truth-value requirement presupposes that both the syntactical and the subject-matter requirements are satisfied: you don't make a true or false statement unless you are talking about something via a certain sort of collocation (*symploke*, etc.) of words.

Next, let us turn to Plato's illustrations of true and false. He illustrates a true statement with 'Theaetetus is sitting' and a false one with 'Theaetetus is flying', Theaetetus being something present, something existing. In both cases, in other words, he chooses *logoi* whose subjects are satisfied, thus keeping in mind the subject-matter condition. And he says that the true statement is true by virtue of the fact that it states something which holds of Theaetetus, and that the false statement is false by virtue of the fact that it states something which does not hold (*hetera*) of Theaetetus. (Cf. *Tht.* 189c.)

Again, Plato says, both in our passage (263b) and elsewhere (e.g. *Crat.* 385b, cf. *Ethd.* 283e-4a), that the true statement speaks of things that are *as* (*ōs*) they are; and he also says (*Cr.* 385b 8) that the false statement speaks of things that are as they are not. This tiny word 'as' does, I think, marvels. It brings out, and brings out strikingly, what I have called the subject-matter condition. For it says, correctly, that a falsehood—and, more obviously, a truth—presupposes that you are talking about something, that the subject of the false statement is satisfied. Otherwise you don't even begin making a false statement, or indeed a statement (cf. 262e, 3c). The only thing is that a falsity does not characterise something the way it is—as it is. And this is what distinguishes it from its corresponding truth, the only thing that distinguishes it from its corresponding truth. Falsehood

is usually dealt with derivatively: it is sacrificed to truth. But it is perhaps on account of the fact that Plato made, or was forced to make, out of falsehood a separate problem—that he came face to face with falsehood itself and not, as usual, via truth—that in my opinion he succeeded where others failed.

Suppose I were to say in a room with no chairs 'the chair is dirty'. Your immediate and natural reaction would not be, 'no', 'false', 'this is not true' and the like, but, 'what are you talking about: there is no chair in this room'. This brings out the fact that a necessary condition for our using ordinarily 'false' ('no', etc.) is, roughly, that the subject of our statement must be satisfied, that unless this is so we wouldn't be making a statement, true or false, but idle noise,<sup>1</sup> or a joke or etc.—which fits in nicely with Plato's analysis. Indeed, Plato does not illustrate a false statement with something comparable to my chair-example. He does not say, e.g. 'the king of Athens is bald'. It is obviously a pity that Russell *et al.* had not read the *Sophist*. Cornford of course disagrees. He fathers on Plato this central mistake of the Theory of Descriptions. He says in effect that according to Plato utterances not fulfilling the subject-matter condition, e.g. 'The present King of France favours Free Trade', are false (PTK, 313).

Now, digging deeper into the spirit of our passage, the difference between a falsehood and its corresponding truth is not to be looked for in the subject, nor, though this might mislead, in the statement as a whole; but rather in the predicate—in what you say (*legein*) about whatever it is you are talking about. And the saying is performed with the predicate not the subject. The latter fulfils the subject-matter or identifying (*onomazein*) condition, and in so doing enables the predicate to fulfil its own peculiar function. In Plato's examples of true and false statements—'Theaetetus is sitting', 'Theaetetus is flying'—what is different is the predicate, not the subject. The subject is common. So what accounts for the truth-value difference is due to the predicate.

It is noteworthy, further, that Plato's illustrations of truth-value are neither analytic statements, nor statements whose subjects are general terms, nor generalisations, nor molecular statements, nor etc., but empirical, singular, atomic statements. Cornford says that the reason why Plato uses a statement about a particular—Theaetetus—instead of one 'about Forms' is

<sup>1</sup> Cf. Cratylus (in *Crat.* 429e3-430a5): If A were to address B in a manner appropriate to C, A 'would be making an empty noise (*psosein*), would be setting himself in motion to no purpose (*maten*)'.

because Plato's opponents would not have found the Form-statement acceptable, the reason being, according to Cornford, that 'they would have denied the existence of Forms' whereas 'that Theaetetus exists . . . is common ground with his opponents' (*PTK*, 309). Cornford, no doubt, says this because in his interpretation of our passage he invokes the absent Theory of Forms. Be that as it may, instead of speculating on Plato's motives for choosing the examples he did, I shall merely state that his choice seems to me a very happy one. For one thing, it seems to be superior to the choice of other theorists, *e.g.* to mention just the most famous one, to Tarski's example of a true statement, 'snow is white'. For the truth of Tarski's example depends on the truth of singular statements, on statements exemplified by Plato's specimen of truth—'Theaetetus is sitting (at time *t*: tense is stressed by Plato himself, *nyn* 263a8)'—whose subject is a proper name, as against the subject of Tarski's statement which is a general word, 'snow'. And so Tarski's, as against Plato's, statement, applies to the purported subject-matter indirectly, via those other, singular statements. (Plato's 'man learns', as Burnet, *Gr. Phil.* 287, translates *anthrōpos manthanei* 262c9, is not offered as an example of a true, nor of course of a false, statement, but of a logically legitimate array of words, of a logos. Otherwise it would belong to the company of Tarski's statement being, like his, a generalisation or, if you will, an omnitemporal statement.) If, on the other hand, Tarski's example is taken as an analytic statement, for strangely enough it can be taken either way, though Tarski doesn't seem to be aware of this, it defeats, I think, Tarski's purpose, which is to elucidate the use or notion of truth as pertaining to empirical not *a priori* statements; hence also the label he chose for his account of truth, *viz.* the 'semantical' conception of truth.

Now the interesting thing is that, assuming that higher-order statements, including 'existence'-statements, have truth-value, Plato's analysis applies also to them. For though his examples of truth-value are statements whose subjects refer to a first-order something, what I have called, and advisedly so, his subject-matter condition does not require that an utterance to be true or false must be about such a thing. All that it requires is, to use Plato's own words, that it be about something. The subject-matter of a higher-order statement is not of course a Meinongian entity, but a concept or the job—or jobs—of an expression or the like. If this is true of Plato's analysis one can readily see that it is misleading to call it a form of the Correspondence or of the Semantical Analysis of Truth. If we want a label for it we should

perhaps call it The Subject-Matter Analysis or, better still, The Subject-Matter Elucidation of Truth-Value. It is worth pointing out that the subject-matter condition, thus construed, is reminiscent of the Stoics' fundamental notion of 'something' (*to ti*). For it, like the condition, is a logico-linguistic or epistemological notion in the sense that it involves talk, statement, thought and the like, *i.e.* the notion of aboutness; and, what is more to the point, it involves also the notion of different levels or orders of aboutness. (See *e.g.* Sextos *Outl. of Pyr.* ii. 223 ff., *Against Phys.* ii. 218, 234, *Ag. Eth.* 224.)

That Plato's analysis applies to 'there is'-statements of the form 'there is (isn't) a mouse in here' is evident from what has already transpired before the preceding paragraph; the subject (in the by now familiar sense of 'subject') of this statement is not of course 'a mouse'—a substance expression—but 'in here', a place expression. Relational statements too can be accommodated in Plato's analysis, only that the elucidation of the truth-value of these is, perhaps, more complicated.

I am not necessarily maintaining, with Russell and others, that the higher-order use or elucidation of 'to exist' is the only one; nor that Plato did successfully cope with existential, as against attributive, statements, but rather that his present *analysis* can accommodate the former without postulating a Meinongian Realm of Being. If so, Quine's 'Plato's beard' need not be Plato's.

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### III.—SENSE-DATA AND MATERIAL OBJECTS

BY NORMAN BROWN

DISCUSSIONS of the problem of perception in terms of sense-data are not as common as they used to be. My purpose is to suggest that the main reason why the sense-datum approach has proved sterile is that it has been made to serve a dual purpose, the two parts of which are mutually incompatible; and that if we restrict ourselves to one of these purposes and re-define the sense-datum accordingly, it may still be fruitfully used as a basic concept in the empiricist analysis of material objects.

The historical genesis of the confusion lies in the fact that the early empiricists were saddled with representationalism. Locke's primary concern was to insist on the source in sense-experience of all our ideas of the material world. That the direct object of that experience was, in some ill-defined way, our own ideas was an additional and embarrassing doctrine which he would have been happier without and which was probably simply the result of verbal confusion. The damage, however, was done; and when Berkeley and Hume came, each in his own way, to discard representationalism, it was not enough simply to lop off the offending and inexperienceable objects. We may concede to Dr. Luce that Berkeley was not an idealist in intention—for did he not insist on the objective reality of our sensible ideas? But you cannot make shadows do the work of substances simply by talking about substantial shadows, and Berkeley should have seen that no sort of an objective world could be built out of elements that still retained the essential characteristics of "ideas"—not least, whose *esse* was *percipi*; so he was an idealist after all. For similar reasons, Hume ended in his unwilling scepticism. Being more clear-sighted than Berkeley he did not try to make his "impressions of sense" into a quasi-substantial world, but merely to uncover the psychological mechanism whereby we come to believe that such a world exists. But if he had not been the heir to representationalism, he might have examined more closely the exact nature of those elements of which our experience is composed. His scepticism may be the outcome of pushing sensationism to its logical conclusion, but sensationism is the child of representationalism, not of empiricism.

The modern sense-datum philosopher is, it is true, unlikely to adopt a consciously representationalist position ; the refutation of representationalism is the first thing the undergraduate learns. But representationalism, like many other fallacies, is easier to refute than to avoid, and it is virtually because the modern sense-datum analysis has in its own way succumbed to it that phenomenalism has proved to be as unrewarding as its predecessor. It is here that the dual purpose comes in which we mentioned above.

Probably the chief attraction of the sense-datum terminology has been the appearance—one may say the illusion—it gives of a cool scientific neutrality. Its use, as Professor Price asserted (in *Perception*, pp. 18-19), does not commit us to any particular view about the persistence, ontological status, privacy or publicity, origin or relation to material things of sense-data. But unfortunately it is not definition which leads to confusion, but the lack of it. It is perfectly proper to use a verbal symbol as we use a mathematical  $x$ —as an unknown to be given a value in the course of the argument. But in mathematics problems tend to be more clearly defined one from the other, and we encounter no difficulty from the fact that  $x$  has a different value in each problem. In philosophy, on the other hand, it is neither difficult nor uncommon to find oneself discussing two problems at once and assuming that one's variable has the same value in each. This seems to have happened with the term 'sense-datum'. For it has been used in at least two quite distinct problems—the problem of differentiating between veridical and other (e.g. illusory, hallucinatory) perceptual situations, and the problem of analysing our experience of material objects from the point of view of empiricism. In the case of the first problem, it has been assumed that there is a common element in our veridical, illusory and hallucinatory perceptual experiences—namely, the experience of a sense-datum; some would extend this also to our experience of after-images, and even dreams. Now there is nothing wrong with this. There plainly is a common feature of some sort in our veridical and deceptive perceptual experiences, or else deception would never occur. But the point is that our variable, however little we may yet know what value to give it, is now tied down to one problem. We may not yet know, for example, what is the precise ontological status of our sense-datum, but it is clear that the possible answers to this question are severely limited by our definition of the sense-datum as something common, e.g. both to veridical and to hallucinatory experiences, for whatever the two types of situation have in common is, *prima facie* at least, something private to the percipient and not public.

It should therefore be clear that if we now turn to our second problem and attempt the analysis of material objects in terms of sense-data (or, if you prefer, the analysis of material-object sentences in terms of sense-datum sentences), our basic term is no longer free from assumptions. Whether or not our sense-datum is a successor of Berkeley's ideas and Hume's impressions—a relic, in other words, of representationalism—we find ourselves saddled with precisely the same problem. For what the phenomenalist analysis has tried to do is to analyse our experience of material objects into an experience of something whose *esse* is *percipi*. True, it was never intended that the sense-datum should be so limited; the supposition that the term implies nothing as to the ontological status of the immediate data of our experience leaves it an open possibility that givenness to sense is merely an accidental, and not the defining, characteristic of what we experience. The trouble lies in our confusion of problems; for the very characteristics which material objects are supposed to have and which the sense-data with which the phenomenalist works appear to lack are those characteristics which had to be eliminated from the sense-datum in order that it might serve as the common element in veridical and non-veridical perceptual situations. The one thing which is common to the real and the hallucinatory dagger, apart from specific sensible characteristics, is the fact that we experience both, and therefore the only way in which we can define whatever is common to both experiences is in terms of its givenness. This is to say precisely that its *esse* is *percipi*.

The results of this error are the well-known failings of the phenomenalist analysis. Since the only actuality (*esse*) which belongs to the sense-datum is its actual givenness (*percipi*), the actuality of the unsensed material object is reduced to the status of possibility (possible sense-datum, permanent possibility of sensation, unsensed sensible<sup>1</sup>); our categorical statements about it are reduced to hypotheticals; the single material object, which is, however mysteriously, a unity, dissolves into an infinity of perceptual occurrences. But it is now recognised that, even if material-object sentences entail an infinite number of hypothetical statements about possible experiences, they cannot be reduced to a series of such statements without remainder. But this is nothing other than Hume's conclusion that we cannot derive our notion of material objects from experience if experience

<sup>1</sup> I am not suggesting that all users of these expressions deny the real existence of tables which are not now being sensed; it is sufficient that they think we can *talk* of them only in terms of possibility.



is interpreted solely in terms of the awareness of sense-impressions.

We may note by the way that even the revised view, as commonly stated, contains an exaggeration. Many material object sentences do not by themselves entail a series of hypothetical statements about possible experiences, but only in conjunction with certain other general statements about the nature of perception whose truth we take for granted and therefore leave unsaid. But a premise can only be said to include a conclusion as part of its literal meaning if it entails that conclusion by itself. A material-object statement therefore need not *mean* any of the sense-datum statements which it jointly entails; though the fact that a material object exists may *mean* that I shall have certain experiences in a non-logical sense of 'mean' (just as the fact that it is raining may *mean* that there will be no play at Lord's today). The exception to this is perhaps those statements which include the attribution of secondary qualities. Indeed, we might define a secondary quality by saying that where a material-object statement of the form *fa* entails one or more statements about possible sense-data, then, assuming *f* to be a simple sensible quality, *a* is a secondary quality by definition. There is no space to discuss this suggestion here; but it may be worthwhile to point out the danger, when considering primary qualities and material-object statements as such, of confusing 'M entails one or more S', where M is any material-object statement and S is any statement about actual or possible sense-data, with "'A utters (believes, etc.) M'" entails one or more S'.

Some sense-datum philosophers have spoken of sense-data not as composing or constituting, but as belonging to or related in some other way to, the material object. On this view a hallucinatory datum is one which does not belong to any material object at all. But such a view cannot escape the criticisms which lie against representationalism, of which it seems to be a modified form; indeed, we are faced with the additional difficulty of accounting for the ontological status of data which are neither parts of the material objects nor ideas in the percipient's mind.

At this juncture we seem to be in an impossible position: each theory we take up under pressure of criticism is as untenable as the last. If we start, for example, from the view that the distinction between veridical and other types of perceptual situation is to be found in the relations between sense-data—that is, in the internal structure of the total sense-field, then, even if we succeed in finding the requisite discriminatory criteria, our analysis of material objects will be phenomenalistic; and that is

objectionable. If, on the other hand, we hold that the material object is something more than, or other than, a family or system of sense-data, then two possibilities are open to us. Either the material object is something entirely other than sense-data, or it is composed partly of sense-data and partly of something else.

On the first view, the question of the relation between the sense-data and the material object arises in its most acute form. Presumably the relationship is in some sense causal, but the causal theory has several well-known difficulties to meet. Most obviously, it introduces into our perceptual consciousness an inferential element which is quite foreign to it. We do sometimes use inference to check or supplement our perceptual judgments in case of doubt, but no such inference is of the type we need. For what the theory demands is that we should infer from something apprehended merely as a personal private experience here and now the existence of an objective, *i.e.* independently existing, substantial continuant as its cause. But in the inferences we actually make, our data are already taken to be objective substantial continuants, or at least objective events in a physical realm of such continuants; the inference is merely, for example, to their further unperceived characteristics or to their identity with objects experienced on previous occasions.

If the uses we make of inference in cases of veridical perception do not support the causal-inferential theory of perception, a nest of difficulties for the realist is admittedly to be found in the phenomena of hallucination, illusion and also the apprehension of secondary qualities, whatever, and whichever, these may be. These phenomena will be mentioned in their place; it is enough to say here that, quite apart from the fact that usually when we perceive we do not infer, no occurrence of inference—even of inference directed towards the establishment of objectivity or the reverse—in these borderline or non-veridical cases could tend to show that causal inference is the basis of all perceptual judgment. In the first place, the procedure which I use in order to assure myself of the ontological status of an experience of pink rats already assumes that I am directly aware of at least some other material objects—*e.g.* of the bodies of the persons to whom I appeal for corroboration. Secondly, the conclusion which I wish to establish is that I was really seeing a pink rat—or, if you like, that I was seeing a real pink rat; it is not, as the theory would require, that what I saw was *caused* by a pink rat.

But thirdly, the Berkeleian system has shown us that a generalised perceptual inference need not lead to a belief in independent material objects at all, as long as some cause capable of producing

our perceptual experience is supposed. The only basis upon which we could erect a more detailed system of inference, leading to a belief in the existence of material objects having at least some of the general spatio-temporal properties we attribute to them, is a basis of analogy, which demands that in some cases at least we should be directly aware of the objects themselves—an argument familiar enough in the context of representationalism.

The alternative view, that the material object is composed partly of sense-data and partly of something else, seems to be a compromise which combines the disadvantages of representationalism and phenomenalism. Endless varieties of theory with this general pattern might be held; but the general defects of such a pattern may be fairly expressed in the following complex dilemma. Let us represent that part of the material object over and above the actual sense-data by 'X'. Now either X is partly or wholly composed of possible sense-data, or not. If all or part of X is not composed of possible sense-data—is, in other words, in principle imperceptible—then all the difficulties in the view that the whole of the material object is something other than actual or possible sense-data also attach to this theory with respect to all or part of X. But if the whole of X consists of possible sense-data in the traditional sense, we are back once more with the phenomenalism which we have rejected. There seems to be only one way of escape. We might hold that the material object consists partly of actual and possible sense-data of the traditional sort, and partly of sense-data which have also the properties of material objects for which the phenomenalistic analysis is unable to account; that is, that apart from being aware of certain transitory and subject-dependent phenomena we are also directly acquainted by sense with certain elements having substantial continuance. Now this may well be the case, but it will not help us here. For if sense-data are by definition such substantial continuants, then the assertion which the traditional theory must make, at least in the case of secondary qualities and of the phenomena of hallucination and illusion, that some sense-data are not substantial continuants, becomes a contradiction in terms; whereas if the assertion 'this sense-datum is a substantial continuant' is a contingent statement, substantial continuance not being a necessary property of sense-data, then nothing in our sense-experience could give us the slightest warrant for making it; for by definition sense-data are the only direct objects of awareness, and Hume has shown conclusively that to experience sense-data as such is not to have direct experience of substantial continuance.

## II

The notion of sense-datum must therefore be re-examined. If every possible answer to a question leads to absurdity, the question itself must be wrongly framed. Our question is that of the relation of sense-data to material objects. Since the word 'relation' is here used in the widest possible sense to include everything from causality to the pseudo-relation of identity, the difficulty does not seem to spring from it. In other words, we cannot trace our embarrassment to a failure to consider all the relevant types of relationship. Apart from the relation of identity, which gives us phenomenalism, we have mentioned only causality; but I do not know that any other specific relation has been suggested which *could* serve as the basis of an inference from sense-data to objects. Every other expression one might use seems to be either pointless or question-begging. To say that sense-data "belong to" material objects is simply to say that they *are* related without saying how; while to say that they present, or are appearances of, material objects is simply a way of saying that we can know material objects by their aid without committing oneself on whether presentations and appearances are parts of what is presented or appears or, being distinct, are related to it by some as yet unspecified relation. Nor can we avoid our difficulties by saying that there is no relation, or that there are no material objects; for this is a point which we must take for granted in all our reasonings—not, I think, for Hume's reason but for Moore's. That there are material objects and that in some sense we experience them is the situation we have to analyse; it is not open to us to deny it. And since sense-data are, *ex hypothesi*, the only things that we do experience directly, to deny that there is any assignable relation between sense-data and material objects is tantamount to denying that we can have any knowledge of material objects at all. The only alternative left open to us is to suppose that 'sense-datum' has been wrongly defined. The impossibility of discovering the relation between sense-data and material objects could be explained in one of two ways. Either the expression signifies nothing in the real world, or it signifies two or more things which have therefore become confused. As I have said, the latter is the correct answer.

The fundamental question was: Why has it been supposed that when we veridically perceive a material object, what we are immediately aware of cannot be part of the surface of the material object? Why has it seemed plausible to suppose

that the direct object of visual and tactual awareness is not the surface of the object in veridical cases and something else in illusory and hallucinatory cases, but another sort of particular, namely the sense-datum, in both cases ?

One reason may be easily disposed of. It is based on the logic of the fact that whereas statements asserting that the speaker is here and now experiencing a sense-datum are incorrigible, statements to the effect that he is here and now perceiving a material object are not, and indeed in the case of hallucination are false. In Price's words (*op. cit.* pp. 105-106) "how can a certainly real quality qualify a doubtfully real entity ? Plainly it cannot." Now this argument rests on a mistake. It treats 'certainly real' and 'doubtfully real' as though they signified two different sorts of reality. True, we could not have an *imaginary* quality qualifying a real object—that would be a contradiction, though we could imagine a real object to possess a quality which in fact it does not possess. Nor could we have a real quality qualifying an imaginary object—that is, a real instance of that quality belonging to an imaginary object ; though once again we could have a quality which was really instantiated in other cases imagined as qualifying an imaginary object. But there is no contradiction involved in one's being certain that the quality of redness is being experienced and doubtful about what the redness qualifies—or indeed about whether it qualifies anything. It may be true, as Price says, that "what is *certainly* red is not the material object" (*italics mine*) ; but *certain redness* is not a special sort of redness, and all this means is that we cannot be certain whether the redness does or does not qualify a material object. It *may* be the case that if we are certainly experiencing redness, it certainly belongs to something ; but it does not follow that if we are not certain whether it is a material object which is red, then, whether the material object is red or not, there must be something else which is red and which we know for certain to be red.

There is, however, another argument which carries much more weight. One form of it may be expressed as follows. The surface of a penny is round, but from all except a single linear series of viewpoints the sense-data which we obtain from the penny are elliptical. Now an elliptical sense-datum cannot be part of the surface of an object whose surface is round, and therefore from all but that single series of viewpoints we are directly apprehending not the surface of the penny but sense-data which cannot be identical with the surface. So to suppose that when the penny appears round as well as being round we are actually

directly apprehending its surface is to suppose that the nature of perception alters radically according as we shift our point of view of the object, and this is a highly unpalatable suggestion. Therefore we never directly apprehend the surface of a material object at all. If this suggestion appears equally shocking to common sense, it has at least the merit of providing a single consistent interpretation of veridical perception.

It then seems plausible to apply a similar argument to the case of downright illusion, and even to that of hallucination. In the case of illusion, one would hardly deny that one is presented with *something*, and it is tempting to assert that this mirage, bent stick, object seen double or the like could not be the material object. Even in the case of hallucination, the presentational impact upon the subject is so great as to make it highly plausible to hold that the sense-datum still serves as the direct object of perception, the difference between the three cases being reducible to either a differential structure of the total sense-field or a differential relation of the sense-data to the world of material objects.

Now since what we are directly aware of, in the case of hallucination at least, is assuredly not part of a material object, the doctrine of the basic continuity of perceptual experience from the veridical through all stages of the illusory to the hallucinatory makes the traditional sense-datum analysis inevitable. Since the sense-datum analysis is unsatisfactory, the doctrine in question must be mistaken. I propose to deal with the question of hallucination by holding that the perceptual experience involved is fundamentally distinct in kind, and with that of all other types of perceptual situation by holding that the material object, with certain qualifications, is after all the direct object of our experience.

### III

When I hallucinate a pink rat, what exactly *is* given? Suppose we say 'a pink sense-datum'. We are agreed that what is given is not a real rat—that is to say, not a rat at all. But this is just the trouble. What sort of thing is a datum? A datum is simply something given; givenness is an attribute and needs a subject. Pinkness may of course be given; there is certainly some sense in which one attribute can possess another. But pinkness is itself an attribute, and must therefore itself have a subject; neither givenness nor pinkness is by itself sufficient to make a particular. A number of suggestions has been made to get over this difficulty, *e.g.* that what is pink is a visual field or

part of it, or the surface of a region of space, or an expanse or extent. But none of them will do. Doubtless we can say all these things, and say them truly; but the pseudo-particulars to which the pinkness is alleged to belong always turn out in the end either to be parts of the physical world or else not to be particulars. Expanses and extents are abstractions—if there must be something which is pink there must equally be something which is extended or expanded. Regions of space, and visual fields, on the other hand, must either be concretely conceived as parts of the physical world, or else regarded as ideal systems of relations and quite incapable of being pink or any other colour.

The shocking truth is that in hallucinations there is no *datum*—nothing is given at all. If there is really no pink rat, then there is really no-thing which is pink. After all, is not that the percipient's whole misfortune? Do we not assure him that there is *nothing* there? Is he not mistaken in judging that there is a rat, and is he not therefore equally mistaken in judging that there is something pink? I am not, of course, denying any of the empirical facts—even that, in some sense of the word 'sees', the man is *seeing* a pink rat. But why, if we can speak of a man "seeing" a rat when there is no rat, cannot we speak of his "seeing" a pink rat when there is nothing pink? To be sure, the delusion is uncannily convincing; he may know—his intellect as well as his friends may tell him—that there is no rat, and still he is unable to banish the vision. But it is not as if the vision were confined to the traditional sense-data; he is not just "seeing" a pink shape, he is "seeing" a rat, a three-dimensional physical object; and I cannot see why it should be more difficult to allow that he can "see" sense-data where none exist than that he can "see" material objects where none exist.

There is a variety of ways in which the point of this manoeuvre may be brought home. It may be observed, for example, that those who describe the hallucinated "object" in terms of sense-data are using language which seems to embody two conflicting points of view. *Givenness* is at best a peculiar concept, not at all so simple as it looks at first sight. If the analogy of the gift has any appropriateness, it is presumably intended at least to signify the distinctness of donor from recipient—here the objectivity or independence of the perceptual object in relation to the percipient. From this point of view the notion of a perceptual datum involves not merely the experiencing of certain modifications of sensuous consciousness by the percipient, but the apprehension of the subject-object relationship by the percipient



—of the existential otherness of what is given. The normal type of this apprehension is, of course, not hallucination but veridical perception. Indeed, the relationship involved in it is one which may be perfectly well, though only partially, described by a third party. The *datum*-terminology, therefore, logically presupposes a veridical perceptual situation, and only appears appropriate to the hallucinator *in so far as he is misled by his hallucination*. It is precisely the givenness, and not the sensory qualities, of his experience about which he is mistaken.

It will be objected that the experience is in a perfectly real sense given to the experiment ; he has no control over it ; he is passive to it much as with veridical perception. But here we may draw attention to another frequent shortcoming of theories of perception. Too often perception is spoken of as though it were the appearance before a disembodied consciousness of a sort of panorama of sensation. Often enough, indeed, the word 'panorama' is only too literally appropriate, for the sense of sight is frequently taken as the model to the exclusion of the other senses. If we regard sensation, still worse perception, as a procession of sensory figures before a passive onlooker, then it is natural to regard the deceitful procession of pink rats as "given" to consciousness precisely as the more honest procession of real tables and chairs is given. But surely the truth is otherwise. The percipient is not a disembodied consciousness but a self-conscious physical organism. In the act of perception a man is perfectly aware—perhaps most obviously in the case of touch—that sensation is a process of his physical organism through which perception is mediated. A hallucinating subject may quite well be sufficiently rational to discover the unreliability of his sensations by empirical verifications in principle similar to those which he would employ in cases of illusion ; and he is quite capable of realising that his mistaken belief in the presence of a perceptual datum was due to a misbehaviour of his sensory apparatus, just as I may soon realise that the blur in my field of vision is not given to me by the external world but is simply a scratch on my spectacles—a flaw in the instrument by which the external world was intended to be faithfully conveyed to my consciousness.

In other words, the application of the word 'datum' to the indubitable element in perceptual or quasi-perceptual situations is misleading, not only because in cases of hallucination nothing is *in fact* given, but because the subject, however indubitably he enjoys a pinkish experience, is not psychologically bound to regard the experience as objectively given ; and the reason is that

the subject-object relationship is one which holds not between the subject as mere consciousness and the modifications of his sensory consciousness, but between the subject as self-conscious physical organism and the rest of the physical world. Hence any sensory experience to which nothing in the real world corresponds may be not only empirically proved but psychologically accepted by the subject to be subjective and not given at all. The fact that the percipient's body is also equally a part of the objective physical world, and can itself be examined by the percipient with a few purely physical limitations, plays of course a large part in bringing this useful state of affairs about. We may express it in this way : sensations are not the objects of which I am aware but the ways in which I am aware of objects ; and the curious fact that I can appear to be aware of objects when there are no objects to be aware of is not made any easier to explain by saying instead that I am aware of actual sense-*data* when there is nothing discoverable given.

All this, of course, presupposes certain views about the fundamental aims and methods of epistemology. I suggest that those who persist in holding that the hallucinating subject is being presented with a *datum* are still under the influence of Cartesianism, not only in their isolation of consciousness from the world of organisms in which it is in fact immersed, but also in their conception of epistemology as the validation of our perceptual knowledge through the analysis of our ideas. On the contrary, the only proper basis for epistemology, in my view, is the acceptance of our direct awareness of the existence of material objects as the fact to be analysed. The history of epistemology from Descartes to Kant has shown conclusively that if we deny a direct experiential knowledge of a world of objects having at least in general the sorts of properties we attribute to them, no amount of theorising will enable our claim to that knowledge to stand the scrutiny of reason ; yet nothing less than that claim will satisfy the ordinary man, whose convictions still remain to be explained by the philosopher.

The point may be reinforced in many other ways ; we may, for example, apply to the case of hallucination, *mutatis mutandis*, the point which Professor Ryle has made about imagination. Hallucination is not, any more than imagination, a matter of sensing a peculiar sort of object, but rather of undergoing, in this case passively, a peculiar sort of process. Instead of seeing pink rats, I hallucinate them ; and this does not involve the presence of pink rats or of any other objects, any more than my imagining pink rats involves the existence of another special

class of objects called mental images. Or we may appeal to the distinction between the act-object and adverbial analysis of perception, and say that whereas the proper analysis of veridical perception is of the act-object type, the proper analysis of hallucination is adverbial; I am in fact sensing pinkly this time, although I mistakenly suppose myself to be perceiving an object.

So far, however, we have given no exact statement of the difference between a veridical and a hallucinatory experience. What the two have in common is undoubtedly their sensible characteristics. For Aristotle, the objectivity of sensible characteristics lay in the fact that they not only qualified the experience but were the form of some substantial existent, the form being conveyed to the mind but not the matter. Different in many ways as was Berkeley's conception of matter from Aristotle's, it also served for him to mark independence of the perceiving mind, and his criticism of the concept as containing no experienceable characteristics tells equally against accepting the Aristotelian version as an element in perceptual, as opposed to metaphysical, analysis. But we need some concept to take its place, for objective existence is not guaranteed to our percepts merely by virtue of the appearance of sensible characteristics in consciousness; the human mind is quite capable of remembering, imagining, hallucinating or dreaming of an object that does not now exist.

The answer is already before us. We have only to rid ourselves of the superstition that objective existence has to be inferred, to realise that it is precisely *existence* which completes our concept of the datum. What we are aware of, over and above the specific sensible differentiations of the object, is its existence as an object, an existence given to us not simply in the relation between consciousness and sensible quality but in the impact of the external physical world on the sensitive organism. The awareness I have of a touch-sensation when my hand strikes the table is not the whole datum but an abstraction only; what I am totally aware of is the impact of my body upon another body. What I am aware of is some *thing* hard. In veridical perception, it is not that I am aware of a touch-datum which in fact has some relation to a real table; I am aware-by-touch of a table. In hallucination, it is not that I am aware of a similar touch-datum which in fact has no relation to a real table; it is less misleading to say that I *think* I am aware-by-touch of a table, although really I am not, as I can quite easily discover. In the first case, I am presented with a hard existent, a real instance of hardness; in the second case I am not presented with anything at all,

though I think that I am. Hardness may be said to qualify my consciousness in the peculiar way in which sensible qualities do qualify consciousness (*intentionally*, the scholastics would say), and not in the way they qualify existents. But this does not imply that there *is* anything which is hard, and only an existent can be said to give or be given ; if a quality is given there must be an existent donor to transmit it. It is misleading to speak of the sensible quality as 'real' except as it qualifies a real existent, so that even the premise of Price's argument was unfortunate. When I hallucinate a pink rat, I am not experiencing a certainly real quality, for nothing becomes real simply by being the object of awareness. My experience is real, if you like, in the totally uninteresting sense that I am actually having it ; but the most that I can say after that is that I certainly *seem* to be seeing something pink, although in fact there is no pink thing there. To say that the pinkness is certainly real, if this is all we mean, is pardonable ; but to say that it is real in the sense that even in hallucination there is something there as an object of my experience, is to say that it is real in a sense in which a hallucinated quality is precisely not real but hallucinatory.

It may be objected that I have made my case unfairly plausible by substituting a prejudice for tactual experience for the prejudice for visual experience. We are certainly not so obviously aware of the aspect of physical impact in the case of sight, for example, as in the case of touch ; and it may well be that our tactual experience plays the more important part in our recognition of the subject-object situation. But this does not affect the issue. We must not be misled by the fact that in tactual situations there is a more obvious continuity of the sheerly physical processes of sensation between our bodies and the physical environment ; this merely helps to emphasise, though it must not be confused with, what I may call the *epistemological confrontation* which characterises perceptual situations no matter which of the senses is employed. Indeed, even in visual sensation we are at a very early age cognisant of the part played by our own physical organs in sensation—as soon, for example, as we realise that our ability to see is connected with the opening and closing of our eyes ; and in general the discovery of the more detailed scientific facts about the impact of the environment on our visual and other sense organs follows on naturally as a part of the general programme of discovery about the nature and behaviour of the physical world which constitutes the whole purpose of sense-perception. Of course, the recognition of the subject-object situation cannot be *dependent* on our realisation

of the part played by our physical sense-organs, for the latter presupposes the former. Yet that element in the recognition of the subject-object situation which I have called the specifically epistemological confrontation cannot be divorced from the context of purely physical confrontation of which the impact of the physical world upon our sense organs is one aspect ; it does not occur by itself, but in and through every strand of physical commerce between the organism and its surroundings to which the senses of the organism have access. The central point is simply this : we experience objectivity and do not infer it ; and this can be accepted only if perception is set squarely in its context of physical interaction.

The occurrence of hallucinations, however, may seem to create an insuperable obstacle for this theory ; for we have to admit that a putative experience of objectivity may turn out to be illusory, while holding that in veridical perception the experience is not only direct but on occasions at least, if the plain man is to be vindicated, self-certifying. The sense-datum theory avoids this paradox by denying that the experience of objectivity is direct or self-certifying, and by divorcing the experience of sense-data, which is held to be incorrigible, from the question of objectivity, so that the illusory certainty engendered in hallucination may be referred to the fact that we indubitably are experiencing sense-data. To this I can only offer the following tentative and telegraphic reply : (1) Some perceptions must be veridical since the distinction exists between veridical and illusory situations ; and we can discover beyond all reasonable doubt in a particular case whether a perception is veridical or not by a series of well-known empirical tests—such as checking up with other senses, asking other people, observing whether the object obeys the laws which govern physical objects, and so on. (2) If there is a valid criterion of objectivity, it cannot consist in the presence of any particular sensible quality, intensity of quality, combination of qualities or relation of qualities. For there is no limit in principle to the complexity, detail or intensity of hallucinations ; moreover, the hallucinatory experience is questionable not in point of sensible qualities but precisely in point of existence, so that we should expect any recognisable subjective difference between a hallucinatory and a veridical experience to be unique and irreducible to sensible differentia. (3) The fact that certain perceptual experiences of whose objectivity I was subjectively convinced turn out to have been hallucinations does not by itself imply that no perceptual experiences could be self-certifying or incorrigible. (4) We may therefore hold that in at least a few

cases of veridical perception we just *are* certain that we are directly aware of a real existent, in much the same way as it has been held to be certain that we are actually having a pinkish sensation when we think that we are. But I do not see how this could be proved, even if it were true; and I must leave the matter there.

#### IV

I have been insisting that what is given in veridical perception is a physical existent—that is, an existent having sensible qualities; or, as I should prefer to say, an existent having sensible modes of operation. If we wish to use the term ‘sense-datum’ for the simple elements of the given, we may redefine it as a single simple sensible mode of the operation of a physical existent. In other words, our experience of material objects should be analysed in terms not of simple sense-contents but of simple sensible modes of existents, though we shall see that the term ‘simple’ is misleading.<sup>1</sup>

My final task is to discuss the phenomena of perspective and illusion. I shall try to show that they offer no obstacle to the assertion that the real modes of physical objects are the direct data of perception.

I have said that the chief significance of the ‘datum’ metaphor in the analysis of perception is that it draws attention to the objectivity or substantial independence of the epistemological object in relation to the percipient. In other respects the metaphor is seriously misleading. Two of these may be mentioned. (1) If I give someone a book, the giving does not normally affect the nature of the gift; the description of the matter of the gift would be identical with the description of the book as it was in my possession. But we should not expect to be able to describe or define a physical object and a sense-datum in the same way, for a sense-datum is but one aspect of a physical object. A visual sense-datum is the object *seen*, and from one particular angle at

<sup>1</sup> For the rest of this paper I shall use the term ‘sense-datum’ in its revised connotation, except that it retains its traditional connotation when it appears in double inverted commas.

Of the many problems we must ignore here, one is that of imperceptible entities, such as atoms; but this does not seem to press more hardly upon us than upon other empiricist theories. Another is the relation of the simple modes to the substantial entity to which they belong. But it seems to me that there should be no difficulty in giving a thoroughly empirical account of the traditional notion of substance on the basis of what is here said.

that. Givenness to sense is somewhat analogous to a gift given only on condition that it is put to one certain use—what the recipient receives is truly part of the nature of the thing but not the whole nature. (2) Givenness to sense is nearly always the presentation of an object in a context of other objects, and even the simple-seeming presentation in the psychological laboratory requires an assemblage of machinery to produce. If sense-data are simple sensible modes of operation of objects, the objects composing the perceptual environment mutually determine their operations, both upon the senses and otherwise. Thus the stick appears straight in air, bent in water, doubled by reflection and so on, while without some luminous body it does not appear to sight at all. A particular "simple" sense-datum, therefore, must sometimes be regarded as the operation upon our senses not of one object but of two or more.

Material objects, therefore, and even the simple modes of material objects, are not given unconditionally to perception; they are given (a) partially and (b) contextually. We have here to overcome another stubborn seventeenth century prejudice which still bedevils us—the assumption, namely, that the given must be a complex divisible into given simples. But sensational atomism is simply another fruit of representationalism. Of course our complex ideas can be analysed into simple ideas, for to form an idea of some quality, *e.g.*, is simply to think of that quality in isolation or abstraction, and it is obvious that the very ability to recognise an idea as complex involves the isolation of its simple parts. We can even isolate conceptually the extension and the colour of a surface. It has been supposed, however, by confusion of idea = concept with idea = sensible presentation, that complex sense-presentations (material objects) can be analysed into simple sense-presentations—complex data into simple data, just as complex ideas can be analysed into simple ideas. But are there such simple data? That there may be some I have no wish to deny; if anyone likes to hold that the taste of sugar or the smell of a rose is conceptually simple, I shall not argue with him. I wish only to maintain that the great majority of data are conceptually complex: the most homogeneous colour-patch not only has both colour and shape but is not even simple in regard to colour; for colour has both hue and value, the latter, unlike chroma, being a distinct quality and not merely a degree of hue. The simplest datum of touch, likewise, has hardness, or degree of resistance, as well as temperature, while the datum which results from the movement of the finger-tips over a surface has certain properties of dimension and texture as well.



The simplest aural datum normally has position as well as pitch—and so on. In short, philosophers from Locke onwards have been thoroughly vague as to what they meant by a simple idea of sense—the point being illustrated by Locke's vacillation about whether the ideas which are given to sense are originally simple, and by his successors' habit of using phrases like 'red patch here now', whose air of rock-bottom simplicity conceals a failure to make it clear precisely what sort of simplicity "sense-data" are supposed to have. Conceptual simplicity they have not, and once this is recognised it becomes easier to recognise also the internal complexity which is introduced into sense-data by the factors of partiality and complexity at present under discussion. Colour-data, for example, may be affected by contextuality, so that, *e.g.* what would traditionally have been called a dark green datum becomes perhaps a light green in shadow; shape is affected by partiality, so that what would traditionally have been an elliptical datum becomes a round datum viewed at an angle.

But are the features of the physical world in fact *given* in this more complicated way? Three senses of the word 'given' in the perceptual context have been recognised. (a) In one sense, we must say that the circularity of the penny is given to all observers, for the physical state of affairs presented to each perceiver is a circular penny. (b) In a second sense, the circularity is given only to observers in specially favoured positions; it is in this sense that we should have said that the "sense-datum" is in most cases elliptical. (c) In a third sense, however, it is the circularity which is given to all observers; for under normal circumstances, no-one who sees obliquely a round object lying on a table will think that he sees an elliptical one.

Now, in the sense in which I have used it, the term 'sense-datum' properly covers all three senses of the word 'given'. I admit that the circularity and the ellipse are given in different senses, but the distinction must, if we are to make sense of our perceptual experience, fall *within* the nature of the sense-datum. The crucial question clearly concerns the third sense of 'given'—namely, whether the circularity of the obliquely presented penny is an immediate datum of awareness. If it is not, the physical givenness of that circularity could never be known. If our datum is no more than an elliptical sense-content, we could not justify our assertion that we *know* the penny to be circular unless we suppose that the observer who views the penny from directly above is, and knows that he is, having an experience of a quite different order in which alone the real shape of the surface is immediately given. But no such radical difference is plausible:

from no special viewpoint am I conscious of a perceptual experience uniquely trustworthy and different in kind from all others. True, it is easier to discern the shape of a surface by viewing it from certain angles rather than others; but it is also easier to discern anything when viewing it from a suitable distance, and in both cases the difference is one of degree and not of kind.

It may be objected that since our apprehension of the circularity of an obliquely presented penny is due to past experience, it is inferential and cannot be certain. I reply by denying that it is a matter of inference. What happens may be suggested by an analogy. Suppose that in the past I have confused the identities of Tom and Harry, who are almost exactly, but not quite, alike. When they are seen together the differences are obvious, and if I have once studied them carefully I can in future recognise them apart, simply because I now know what to look for. I do not now *infer* that this is Tom, for recognition, whatever it is, is not inference. So with the interpretation of sense-data. I *may* first of all think a penny elliptical, and then come to correct my error by seeing it from a better angle or touching it or otherwise experiencing it more closely and accurately. But when next I judge the obliquely presented penny (or another object of similar shape) to be circular, I do not *infer* that it is (probably) circular despite appearances because I have learned that a certain degree of ellipticity from one angle is normally associated with circularity from another or circularity to the touch; I now *see* the object as a circular object obliquely presented, just as I come to recognise by direct inspection that the tramp before me is really Jones in disguise. We must not make the mistake of thinking that any learned distinction must be inferred rather than recognised, for recognition itself can be perfected by practice.

The traditional theory involves also the assumption that the ellipticity really is the primitive psychological datum. But is it? A child drawing a house will draw it rectangular, because it is rectangular and he so sees it; perspective has to be learned. The assumption would presuppose that we start with the eye of a draftsman and have to unlearn our primitive perspectival accuracy in order to make our ordinary perceptual judgments.

If these points are well taken, our three senses of "given" collapse into one. For when our penny is obliquely viewed, what is given is neither simply a circular nor simply an elliptical surface, but a circular surface obliquely viewed; thus what is given (*a*) as a physical fact is given also (*c*) as a psychological datum to perception, for sense-perception is a function rooted

in the physical; while the "sense-datum" (b) which was supposed to be the direct object of cognition and often to differ qualitatively from the material object is simply a philosophical abstraction, not an object of perception at all. Rather it is a *sense-content*, a mode of sensation rather than of the object, lifted bodily out of the complete perceptual situation through neglect, on the one side, of the difference between sensation and perception and, on the other, of the physical context, including the nature and position of the percipient's body, in which the object is embedded.

Whatever we may say of the circularity of the penny, however, it seems hardly plausible to assert that the straightness of the stick immersed in water is equally given to perception; and we need to distinguish here between distortions of viewpoint and distortions of context. Distortions of viewpoint are what we may call physically subjective.

This concept needs elaboration. Since the act of perception involves awareness not merely of the self as knowing subject knowing the physical world as a whole but of the percipient's body in relation to other bodies, the effect of the differential relation of the percipient organism to its surroundings is to differentiate the information received; but the information is still, from the epistemological point of view, objective knowledge. An epistemologically subjective modification of experience, on the other hand, would be for example a hallucination. This distinction incidentally throws light on that between primary and secondary qualities, which needs revision: the so-called secondary qualities should perhaps be regarded as physically subjective only, and therefore as real and objective in the ordinary sense. The fact *e.g.* that I am discovered to be colour-blind throws light on the state of my physical sense-organs; it does not provide an argument for asserting that colour is unreal.

Now, since perspectival distortions are physically subjective, we are not surprised to find that man is naturally able to learn to correct his perceptions in this respect, and to see the penny as round. (Persons who are sceptical of Providence may be surprised, but the fact remains.) Distortions of context, on the other hand, are entirely objective. They arise from the relation of the perceived object to the rest of its physical context, including the observer's body, whose influence may be contextual, as when it casts a shadow, as well as perspectival. Therefore it is again not surprising that, although we come to recognise the half-immersed stick situation for what it is, we do not normally learn to see the stick as straight in the same way as we see the penny as round; for the mind to read off the stick as straight

! would be tantamount to its being oblivious of the water, which is equally part of the perceptual object. Similarly, we do not recognise a dark green as a light green in shadow unless we see it in the context of a shadow-throwing object.

At first sight this concession seems to re-introduce the need for "sense-data" of the traditional type; we seem to be admitting that in cases of so-called illusion we are confronted with a datum, such as the visual bentness of the stick, which misrepresents, and therefore cannot be part of, the material object. But the visual bentness of the stick misrepresents reality only if we regard it as a presentation of the stick in isolation from its context.

The visual bentness of the stick in water is just as much a *real* operation of stick-plus-water as is the visual straightness of the stick in isolation or its straightness to the touch. I am not denying the obvious fact that in one perfectly clear sense the stick appears to be bent and is not; I am simply denying the propriety of isolating this appearance for the purpose of epistemological analysis and treating it as the relevant sense-datum, for the stick in this case is not the whole of the datum. Once we have made this mistake, our feet are set on the primrose path. For if we regard the bent-stick appearance as a "sense-datum" in some way intermediate between percipient and object, it is difficult not to regard veridical "data" in the same light, so that our direct awareness is inevitably confined to intermediate entities which may, but often do not, correspond to noumenal realities that lie behind. From this it is but a moment before we find ourselves saying: 'It looks straight, it feels straight, and so on—but is it *really* straight?'—a question whose absurdity has been often enough pointed out. But the nightmare of possible total delusion is the penalty the representationalist must expect to pay in the small hours; and here a theory which combines the merits of phenomenism and realism is at its strongest. For part of what we mean by straightness is simply for an object to manifest certain properties to our senses—one body acting upon another; and as a thing acts, so it is. The appearance of bentness, then, is also a real operation of nature upon our senses, but not of the stick alone; and it only requires a sufficient collection of information about the behaviour of the stick in different contexts, and an application of the method of differences, to give us indubitable knowledge of the real nature of the stick, and also of light, water, mirrors and the rest—knowledge which, however far from being exhaustive, is knowledge as far as it goes.

The truth is that the distinction between appearance and

reality, though appropriate enough in the case of hallucinations, is extremely misleading when applied to cases of illusion, for it suggests and fosters the theory that perception is a matter of inference from conceptually simple "sense-data" to real qualities of isolated objects standing in a one-to-one relation to them. In fact these "sense-data" are abstractions both from the reality and from the complexity of what is given to sense; the real sense-datum is conceptually complex, and the operation of which it presents one experientially indivisible mode is seldom if ever the operation of one single object in isolation; the acquisition of perceptual knowledge is the process of learning to discriminate the elements of that complexity. An "illusory" experience is just as veridical as any other as far as the data go. It is in the over-hasty identifications and predictions we make that error lies, and it is these which suggest to us the connexion of such "appearances" with falsity or unreality. To regard a mirror image, a mirage or the like as *unreal* because we are misled by our first premature judgments of what we see is like calling Tom unreal because we mistake him for Harry. In much the same way, a waxwork effigy is not a fraud because we mistake it for a policeman; it is not an unreal policeman but a real waxwork effigy. Only human beings are deceitful; things just are.

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## IV.—THE PSYCHOLOGICAL PRESENT

BY P. MINKUS-BENES

1. Assertion and Proof of the Incorrigeability of statements merely reporting my present feelings :

Bacon said : "*Est vera philosophia quae mundi ipsius voces quam fidelissime reddit et veluti dictante mundo conscripta est, nec quidquam de proprio reddit sed tantum iterat et resonat.*"

When in philosophy we say that the statement "I feel a prick" is incorrigible we are merely asserting a fact known to us all, a fact concerning the language. But that we all know this fact does not entail that we are all capable of describing it clearly, no more than the fact that we all know what we feel when sitting on a wicker chair entails that we can give a true and clear account of it.

In saying that the statement "I feel a prick" is incorrigible we merely iterate and resonate, to use words like Bacon's, such facts in the world as all of us know, even if they are facts that not all of us are capable of describing clearly. It is a fact of language that we all know but hardly ever state and which the dumb and speechless amongst us certainly cannot state, that the sentence "I feel a prick" is under certain circumstances accepted as right and called 'true' and under other circumstances rejected as wrong and called 'false'. Although hardly anyone ever states this we know it. That we do know it is shown by the way we convince ourselves of its truth by a ready memory alone. Once it is said that 'I feel a prick' is under certain circumstances accepted as right and called 'true' and under others rejected and called 'false' we convince ourselves that what is said is true by readily recollecting facts. For example, perhaps first the fact that we would call the words false and reject them, if a lunatic uses them now and also tells us now, that his crown is full of new thorns and this lunatic has been for some time showing in all kinds of ways that he continually imagines himself to be wearing a crown of thorns that pricks him, and that he regards this image as a reality ; and for example, perhaps second, the fact that if a lunatic says 'I feel a prick' and points to his left foot and a thorn is found in his left foot, we should call the words 'true' and for acceptance as right.

It is a more specific fact of language but equally a fact of language that if I make use of the words " I am feeling a prick " in all the ways in which they are used, then I sometimes count my present feeling either as a guarantee that the words are right or as a guarantee that the words are wrong or as a guarantee that the words are both right at a stretch and wrong at a stretch. In other words there is, as a matter of actual fact about language, a use of the words " I am feeling a prick " such that if I now use these words in this sense or use, then I count my present feeling either (1) as a guarantee for, as making certain, as in virtue of mere reflection fully justifying the assertion ' I feel a prick ', or (2) as a guarantee for, as making certain, as in virtue of mere reflection fully justifying the denial of ' I feel a prick ', or (3) as a guarantee for, as making certain, as in virtue of mere reflection fully justifying the words ' At a pinch it is true and at a pinch it is not true to say I feel a prick '. In this use therefore of ' I feel a prick ' I count my present feeling alone as making certain, as settling conclusively, that the words should be asserted, rejected or be regarded as both true and false at a stretch. From which it follows that to me the question whether ' I feel a prick ' is true in this sense, is merely a question for classifying what I am undoubtedly aware of, my present feeling. For if a present feeling is now to be taken as a guarantee of truth or a guarantee of falsehood then no subsequent feeling can now be taken as outstanding evidence, evidence yet to come of truth or falsehood, for the question of truth is conclusively settled, settled now.

It must be made quite plain that what we mean by the assertion of the incorrigibility, is a fact of language. What we mean by the assertion of Incorrigibility is that we count our present feeling by itself as a decisive warranty, as a complete guarantee of its acceptability or inacceptability or its dubiety. If I now feel a certain sharp sensation as of a small object hitting and perforating my skin then I count it as an infallible guarantee for the acceptability of ' I feel a prick '. But that I count something as an infallible guarantee for the acceptability of ' I feel a prick ', of a certain form of words, must be a fact about how and when I consider a noise acceptable and such a fact must be one about the language, my habits with it. If I am trained to regard the presence of people blessing the Lord as an infallible guarantee for joining in the blessing of the Lord, this is a fact about my routine of prayer. I can plainly distinguish between the instruction " If you feel a prick it is unconditionally right for you to say ' I feel a prick ' ", and the instruction " If you feel a prick it is usually right to say ' I feel a prick ' but not if you are not quite yourself ", and these



two different instructions define for us two fundamentally divergent uses of "I feel a prick", the one with which we merely report the feeling of the moment and which is the one we are here concerned with and the other with which we assert more. And in the difference between these two rules there lies the difference between an expression such that my present feeling guarantees it true and an expression that can only be inconclusively supported from the present feeling. Acceptance of the unconditional rule is acceptance of certain feelings as guarantees of truth and acceptance of the conditional rule is acceptance of the same feelings merely as inconclusive reasons. But that in principle we can have decisive directives, unconditional prescriptions and so treat certain feelings as guarantees is no more surprising than that we can have prescription and adherence, both unconditional, to any principle you please, for example, that I must not tread on the cracks in the pavement. Such unconditional adherence to such unconditional prescriptions can be brought about by any number of not in the least surprising causes, a caprice, a precept or years of training.

And this fact that there is unconditional acceptability of the words 'I feel a prick' in one of their senses whenever I feel a prick, can be shown to be a fact that we all know in that we are all familiar with it, for don't you remember what it is you do to find whether I truly say 'You feel a prick, don't you?', you remember that in such a case you just consider what you feel and hold this to the point in such a strong way that it would never enter your mind to wait for, wonder over, consult the future. You surely remember that you consider the words unconditionally acceptable provided only you feel a prick, for what else is there that you should remember you go by? You remember then the very rule that we are concerned with when we call the statement 'I feel a prick' in one of its uses "incurrigible". The fact we are concerned with is known to all, no more of a discovery than the other two facts about the words 'I feel a prick' that we gave at the start.

2. All that is meant by saying that the statement 'I feel a prick' is incurrigible, is that in so far as it is used in the sense we are now concerned with, its truth is decided only by the present and hence it is plain that what is not meant by calling 'I feel a prick' incurrigible, is that I should decide what its use is by the present feeling. It is self-contradictory to assert that I decide what use an expression has by the feeling I now have, for it is self-contradictory to regard the use of a word in accordance with a rule as something guaranteed by my present feeling. Hence, if

we had asserted that I now know the use of 'I feel a prick' conclusively or completely by my present feeling we should have contradicted ourselves. But in fact we said something that was true, namely, that in so far as the expression is normally used, the present feeling alone is used to guarantee its truth or falsity or dubiety. If there is no difference between A being in fact used to guarantee 'B' and A guaranteeing that A is in fact used to guarantee 'B' between a present feeling being used to guarantee the acceptability of a noise, and a feeling guaranteeing that it itself is used to guarantee the acceptability of a noise, then surely there is no difference between any one thing and any other. It is a plain fact that the present feeling is made to serve as a guarantee of the truth, falsity or dubiety of the sentence 'I feel a prick' and it is a plain contradiction to say that this fact follows from the fact that I now have a certain feeling. All of which enables us to deal with the objection from verbal corrigibility; let someone say: "Of course I can feel a prick and say 'I feel a prick' and use the feeling as a guarantee of truth of what I say. And moreover, if 'I feel a prick' has that use you are concerned with and which normally no-one disputes it has, then what I say is guaranteed true by what I feel. But what is there about the situation here and now, when I feel a prick that can make me certain that any particular way I use the words is the way the words are normally and properly used in the language? Nothing. For no present can. And if, as is possible for all I now feel, it should turn out that the words 'I am feeling a prick' are used to mean that I am Welsh and in fact I am not Welsh, then it would turn out that the words are false. But it is always possible, conceivable, for all I now feel that the words 'I feel a prick' are used to mean that I am Welsh and I am not Welsh. Hence it is always possible, conceivable that the future should correct or disprove my present statement 'I feel a prick'. So why say the opposite, that the future cannot correct this statement?"

All of which objection proceeds from the fact that, for all I now feel, it could turn out that the words 'I feel a prick' are used in any way you please and its determinate consequence that for all I now feel it could turn out that the words 'I feel a prick' are so used as to assert something that is not the case. And just because this objection proceeds from a truth of logic, this objection cannot be denying anything we said. For we merely said that 'I feel a prick' has one use in which it is so used as to be taken guaranteed true or false or dubious by the present, *a fortiori* so used as not to be confirmed or confuted by what is to come. We are in the position of one, who, having asserted a fact of language and

therefore a fact of nature—performance of a philosophic task, as Bacon ought to say—is considered in the wrong on the basis of something that is true by definition, namely, the truth that my present feeling cannot guarantee the words are not so used as to claim something false. We are in the position of one whose point is being misunderstood. And it is quite illegitimate for us to be misunderstood in this way, for it to be thought that we are saying what is self-contradictory, when we proceed to prove what we say by a fact, a familiar fact of language. For this is made illegitimate by the very fact that we offer a fact as proof. If a man says “ ‘I feel a prick’ is a one-term statement because there are not several bits of successive evidence by which I tell it true”, it is then illegitimate to think that in the first part of his statement he means what is self-contradictory, and so to understand the first part of his statement that it means what is self-contradictory, for he offers a fact as proof. Moreover, it is quite extraordinary to use, as the opponent does, when speaking of a statement and calling it ‘incorrigible’ the word ‘incorrigible’ in such a way as to assert a truth of logic, for this expression is by us specially introduced to mark truth of fact of a statement, to mark down that a statement lacks that external, non-essential property that we also attribute to it with the words ‘has such a use that what I will feel counts beside what I now do feel in now deciding whether it is true or false’. And if we degenerately call ‘not incorrigible’ a statement about my present feelings only, because we degenerately call all statements ‘not incorrigible’ anyway, we invite to say “ ‘I feel a prick’ may be corrected on the basis of future experience, when it is used to state merely a present feeling”. And this is in a way misleading, for it is somehow misleading to call ‘correction’ or ‘disproof’ of a statement that which happens if its use is shown improper, or if its use is shown to be other than we made of it or thought it had, seeing that one is liable to think of disproof as that which is the facts turning out against the truth of the statement in the sense in which it was used. What I am apt to think of as disproof of ‘this is a table’ is the fact that the thing referred to collapses upon touch and behaves as flimsy paper does and not that so many people tell me and prove to me that what I have called ‘table’ is what the hospital register calls ‘surgical desk’. Such things as these are indeed disproof but I am liable not to have them in mind when I speak of disproof, and so I am liable to misunderstand one who says “ ‘I feel a prick’ in its most usual sense is capable of disproof by what is to come” for just that reason that I may think he is telling me the future facts can turn out to be

against its truth in the sense in which it was used. But this which I may then think he is telling me is quite untrue. For when 'I feel a prick' is used in the relevant sense the facts don't come to turn out for it or against it as they are so to speak sitting there at the time and finish the business of being for and against.

That there is a sense of 'disproof' covering instruction in language, only helps to confuse us if we do not see, as one is liable not to see, that 'disproof' in that sense is not an application of facts to come against a statement, against a statement in accordance with the rule that is the meaning of that statement, but is merely an application of future facts to show that the meaning of that statement was misconceived. There is even, at a pinch, a sense of 'disproof' that is merely an application of future facts to show that the meaning of a statement was misconceived as by my own obstinately defended private language. I 'prove' to you that the thing is not a table, not a surgical desk, but merely a repository for instruments of torture, by always calling it that and insisting that I have always done so. Am I not disproving you in some sense? The good verbal disproof and the bad verbal disproof, which one would hesitate to call "a disproof" are on a par in that I am liable to think of neither when you speak of "disproof". Do we chiefly argue, prove about words when what we are concerned with are tables, pricks?

There is not that cardinal form of disproof of 'I am feeling a prick' which there is of 'I am my true self now and feeling a prick'.

3. Bearing this in mind we may deal with the second objection. "I feel a prick" it says, is surely corrigible, seeing the future can at least seem to show that I mistakenly compared the present bumpy and sharp feeling to a prick, in that in the future I can come to see or seem to see that the present feeling deserved the name 'bump' rather than 'prick'.—We answer that in such a case the future re-instructs me in the language after what either is or is not a lapse. So that the corrigibility here amounts to no more than that I am necessarily instructable by facts to come in the language. By no means does the corrigibility here amount to a disproof of that fact that constitutes what we call 'incorrigibility', i.e. the fact that if I use 'I feel a prick' in the most usual way, I take my present feeling as a guarantee of its truth, falsity or dubiety, which fact never prevents me from being re-instructable in the language but prevents me from taking the future as relevant to the truth of the words in the sense I use them.

4. Bearing this in mind we may deal with the third objection. "I feel a prick", it says, is surely corrigible, for if it is a statement

it is made for a reason and its reason must precede it, but it is an assertion of its reason hence about something that slightly precedes it. But an assertion about a preceding feeling may be revealed false or shaken by indications yet to come that memory was out of order at the time of assertion. Which objection is countered by saying that what I do in fact take as the guarantee and meaning of the statement 'I feel a prick' is not that part of the prick that provoked my saying so, but that which I feel whilst I say so. If a prick starts me off saying these words and as I say the words I no longer feel a prick, then I am just unfortunate and wrong in what I say. Which is how luck plays a game with statements about my present feelings. If anybody wants to say that in that case when I start saying 'I feel a prick' I must be talking about the future, he is forgetting that this kind of talking about the future is called "talking of the present", and that the turning up of favourable evidence to support the statements that arises out of this very special kind of talking about the future, is the turning up of evidence now, is my feeling now a prick or a jolt or whatever it is. Or shall we really say that fundamentally every statement is corrigible, in that it lasts long enough for me to be deceived as to what will happen whilst it goes on and has happened whilst it went on? What if we suggest that if we use the word 'prick' instead of 'I feel a prick' and say it as quickly as a fluent speaker does, the time taken to say it is not long enough to be wrong in memory of feelings that happened to be had whilst it was being said and not long enough to be wrong in expectation of feelings that will be had whilst it is still being said? The answer is: the time is long enough; and it must always be long enough. So it seems we could never possibly talk of speaking solely about the present feeling. But this goes too far, and if a line is to be drawn it must be drawn in accordance with humble propriety which would say that as a man says 'I feel a prick' at a normal speed, he is concerned with the present throughout and now feels all that feeling that constitutes reason and guarantee of truth of what he says. Only when the notion 'at one time', 'when' has become the notion of a razor's edge can't we say this. Thus whilst, upon a most precise account it is right to say that as a man starts saying 'I feel a prick' he then does not yet feel all the feeling that makes what he says true, it is also right to say, upon a less precise but often apposite account that as a man says 'I feel a prick' he feels at that time a prick that guarantees what he says true. And only if we begin to speak of what he feels at subsections of the periods only the wholes of which we consider when we speak of language, do we

find we have to say the speaker who says ' I feel a prick ' is at each stage of his performance concerned in a big scheme involving assertion of past and future, and at each stage knows only a bit of noise being made by him accompanied by a phase of feeling. Which sad decline we shall never escape unless we say " ' What I feel at the time ', the words ' What I feel now ', are only to be used so that I can be said to be feeling now and at the time all that makes true my present sentence ' I seem to see white and black ' ". So that this objection gives us the warning that as time is considered sharply, one cannot say these things, whence it must be plain that when we say these things we cannot be considering a very short period of time as ' now '. And if we are asked to consider them, we must say ' this is not how we are reflecting ; would you tell the history of the world by seconds ? ' For we are supposed to be considering language and there is nothing in the least strange in one so engaged saying ' Suppose I now say " I feel a prick " '. But if there is nothing strange in our saying this, then equally there is nothing strange in our saying ' I now feel all that makes my statement " I feel a prick " true '. For if I can be said now to be making the statement I can also be said to have now what is no bigger in time than the statement.

5. The objections we have so far dealt with may be called ' truly reflective '. As many things as there are about the remark ' I feel a prick ' as incline us to say it can be corrected, as many truly reflective objections there are for calling it ' corrigible '. And all these proofs will fail to disprove the statement that ' I feel a prick ' is so used that a man asserts it and takes as conclusive proof of its truth, falsehood or dubiety, the present feeling only, for they will be proofs from other features of the remark " I feel a prick ", and all these proofs are an illustration that a general name in a philosophic reflection is a target for attack as long as the reflection serves a purpose of interest, interferes or helps to interfere pointedly with philosophic conversation. The assertions of incorrigibility through its proof helps to interfere with us by helping to explain a score of metaphysical doctrines, all of them about the Difference between the Given and All Else. And so it must be a target for attack, for what is used to make the talkers explain away so much of their talk must be for scrutiny. Which is why there should be such objections as we have come across. And not one of them makes the least difference to the truth of the dictum that ' I feel a prick ' is so used that I decide the truth of ' I feel a prick ' by the present alone, which entails the dictum that if I have now a feeling that is proper reason for saying ' I feel a prick ' it is a conclusive reason.

Which two dicta in turn remind us of much the same as saying that the question 'Do you feel a prick?' addressed to me is one for decision by, description of the present, mere reflection upon the present. Which three dicta are equal as a mnemonic device to the fourth that the mere rules of language oblige me to be prepared to say on the occasion of feeling a prick 'I feel a prick'. All these four sayings are proved from the same premiss, using the whole premiss or part of it, the premiss that I do and others do *inter alia* so use the statement 'I feel a prick' that what we take as conclusive proof of Truth, Falsehood and Dubiety of these words is the feeling we have at the time.

6. As all objections to calling 'I feel a prick' "corrigible" are reflections and not discoveries about the use of 'I feel a prick' we may leave out the formally explicatory adjective 'reflective' and ask what objections there are beside those that proceed from true premisses about the use of the sentence to conclusions rationally based upon such premisses. And we find that there must be objections starting from such premisses by steps that are confusions. Of which you are now to be given three examples:

1st confused objection. Suppose I now say 'I feel a prick' now feeling a prick and further suppose it later looks to me consistently as though I were a robot's head being experimented on and had always been such a robot's head and that I now had been made to say 'I feel a prick' whilst feeling a prick. Would not such a future count against, and be admitted by me here and now, to count against the truth of my remark and its propriety? Is not the remark 'I feel a prick' therefore one to whose truth there is now relevant my not later coming to have reason to think that I had always been a robot's head? Is it not for correction by what is to come? Do I not now in saying 'I feel a prick' whilst feeling a prick take a risk of being mistaken in that the future might be such as to convince me I was now a robot's head being experimented on and being made to say 'I feel a prick'?

This objection is simply confused. For such a future would not now count against the truth of my remark or at any other time. It would merely count against the truth of the conviction that I am now by my own initiative saying 'I feel a prick' and against the truth of the conviction that I am not now a robot's head. In other words whilst such a future would entitle one to ascribe to others initiative in the remark 'I feel a prick' and would be a proof also against the remark "I make the remark 'I feel a prick'" and would be a proof also against the statement 'I am a human being' it would not be a proof against the statement 'I feel a prick'. For us to think that such a future would



be a proof against the statement 'I feel a prick' we must think 'I feel a prick' claims as much as "I, with initiative, say 'I feel a prick'" or as much as 'I am not a robot's head' or some such thing, but this we will not think if we clearly remember the remark is guaranteed true by the feeling at the time of speech. And to say in particular that 'I feel a prick' cannot be truly uttered by me, if I have not the initiative in saying it, is completely to forget that I can make such remarks truly without the least initiative but upon being drilled to make them or being experimented with. To say "If you put a current through my jaw and pharynx that makes me say 'I feel a twitch' and it gives me a twitch, then what I say is certainly not true because it is not a proper utterance at all" is to confuse the sense of "improper" that is a guarantee only of "not said with initiative and understanding of the speaker" and the sense of "improper" that is a guarantee of "such that it would not be true". For it is plain that 'I am feeling a twitch' in this case is only improper in the first sense and hence is not improper in the sense of 'improper' that guarantees its not being true. For it is plain that 'I feel a twitch' in such a case is only improper in that I have no initiative in saying it and hence not improper in that I do not speak the truth.

And in case we get, in the face of this example, the idea that 'I feel a prick' is to be called improper because 'It feels a prick' or 'A prick is felt' are the only correct form of words to be allowed in a robot's head, we are just mistaken. And this idea I discredit as follows: We don't *have* to object, for linguistic reasons, to a machine's using the first person singular; we don't object for linguistic reasons to the Lord's giving a tree speech in the first person singular without giving it a soul, so why should we have to object to a robot's head being given speech in the first person singular by its makers?

What must be kept apart is that (1) in order to gain a point of distinction and clarity in the language we should prefer robot machines and trees to use 'It' together with the word 'this' and the name of their kind, of themselves, and (2) that the word 'I' is improperly used by a robot. The second is false, the first is perhaps true. And that the second is false you may see from the linguistic tolerance we extend to the use of 'I' from machines and God-ridden trees. And it may be illustrated incidentally from the fact that one who—in paranoia—is strongly confident of being a robot will quite normally call himself 'I'. Or do you think he would not? would not say 'I want my breakfast', 'What, me for this job?' or do you think that a paranoid's sense

of linguistic propriety does not count? But a man can easily make out that things are worse than they are and not go wrong with the language.

And again—if the idea is that if I think all my thoughts and attitudes and actions are necessarily given me by others, I cannot, in consistency with this thought admit I truly use the word 'I', this idea is wrong. I cannot in consistency with this thought admit I truly use the word and with initiative use the word 'I' but this is quite different from not being able to admit in consistency with this thought that I truly use the word 'I' in some other way. Surely I can truly use 'I am a prisoner' if you make me a prisoner and steer all my actions and thoughts with radio waves and make me say 'I am a prisoner'. For I am then speaking the truth. And I can believe all these things about being a prisoner and, consistently with this belief, believe that I truly use the words 'I am a prisoner', only my belief that I truly use the word 'I am a prisoner' cannot possibly be coupled either with the belief that I have the initiative in saying 'I am a prisoner' or coupled with the belief that I have the initiative in believing that I truly use the words 'I am a prisoner'. But these are harmless restrictions, for there is nothing contradictory in my beliefs if I believe I truly use the sentence 'I am a prisoner' and don't believe I have the initiative in believing this and don't believe I have the initiative in not believing I have the initiative in believing I truly use the sentence 'I am a prisoner'.

2nd confused objection. It is said that 'I' when used by me refers to a human person, namely, the person that is me. And if this is not true I do not know what is. And it is said to follow that 'I' when used by me means as much as 'the person that is me', and if this follows then what is false follows from what is true which is impossible. For plainly the sentence 'I feel a prick' when I use it in normal life is so used as to be proved conclusively true by my feeling a prick at the time whilst the sentence 'the person that is me feels a prick' is not so used as to be conclusively proved by my feeling a prick at the time. Which objection is a confusion in which we confuse 'refers to A' with 'means as much as "A"'. Suppose I was and sometimes am satisfied that 'I' when I use it of myself refers to a person of royal origin. It is not true that whenever I accept the proposition that 'I' when I use it refers to a person of royal origin, I cannot withhold assent to the proposition that anything which would disprove 'the royal person that is me feels a prick' would also disprove 'I feel a prick'. For whilst accepting the former, I can still properly insist that any proof to show that certain documents were forgeries goes

against 'the royal person that is me feels a prick' whilst it does not disprove 'I feel a prick'. Which shows that "'X' refers to Y" does not entail "'Y' means what is meant by 'X'". Which may also be illustrated by the example of 'this finger has a cut on it' said referring to my left thumb. For if this is said, its method of disproof is still not identical with the method of disproof of 'my left thumb has a cut on it'. For if I say 'this finger has a cut on it' and come to see that finger as a part of my right hand as my right thumb, I shall have no reason at all to believe that what I said was wrong, and we may quite consistently suppose that the finger I referred to was in fact my left thumb but somehow seen as my right thumb. Yet at the same time if I say 'my left thumb has a cut on it' and come to see the finger I referred to as part of my right hand, as my right thumb, then I shall have some reason to think what I said wrong.

3rd confused objection. It is said that 'I feel a prick' means to you what it means to me, but to you the question of its truth is one for further investigation, hence to me it must be one for further investigation.

This confusion is monstrous beyond words, since in it from the relatively indefinite 'means to you what it means to me' there is inferred the very concrete falsehood that 'Do I feel a prick?' is not a question for decision here and now. What must be meant by saying "'I feel a prick' means the same to you as it means to me" must under no circumstances be that the question 'Do I feel a prick?' is not a question for mere reflection to me, or what would be meant by the words would be false. What is true and what is the proof that 'I feel a prick' means to you what it means to me is that it means to us both the same fact, namely, that I feel a prick. But that it means to both of us that I feel a prick can only be true if I have a way of checking it that you don't have. Hence the very meaning of the premiss of the confusion shows the conclusion of the confusion false and the confusion must be called a self contradiction.

7. It is plain that we have not exhausted the number of confused objections. There is, in fact, nothing to prevent my making new concepts and so to misread their meaning as to infer that 'I feel a prick' has a use such that to its truth the future is relevant. What if, just because 'I feel a prick' has a use such that the present alone decides its truth, I say to sum this up, 'The sentence "I feel a prick" is not an other-factish sentence'. Someone can then come along and say 'You mean therefore it makes only one prediction, but if so it must be corrigible'. We see at once how a new concept in virtue of what it looks to someone to be

can be aligned with notions it was never meant to be aligned with.

When we have seen how many old confusions there are, we have seen how varied the forms of mis-proof of the corrigibility of 'I feel a prick' are. The terms involved in the confusion are liable to be collected from all over. From among concepts of language as in the third confused objection where by misusing instead of using the words 'means to you what it means to me' we infer from them 'is checked up by you as it is by me'. From among concepts of science and concepts of logic as when one argues from 'a prick is by nature a disturbance' to "'I feel a prick' entails 'I feel a disturbance'". From the field of philosophy as when one says "knowledge never includes its object, cannot include it in the state of knowing, hence 'I feel a prick' cannot be known to be true and have its truth included in the state of knowing". In which confusion, "Knowledge" is so defined that it cannot be that which is meant by "knowledge of 'I feel a prick'" and the concept of unembracing knowledge is now illegitimately applied to a new field.

8. When we say 'I feel a prick' in one of its uses is incorrigible, we are asserting a fact. And what makes us say it is not corrigible in one of its uses, is the fact that in one of its uses there is no such thing as the taking back or checking of the remark in view of facts to come, which is so because and only because in that use the remark is decided true false or dubious by the things that are in present attendance on us. There is in one use of "I feel a prick" no such thing as the taking back or doubting of the remark in view of facts yet to come, facts that count against the truth of the remark in accordance with the use of the remark. For there are in one of its uses no such things as facts to come that count against the truth of the remark in accordance with the use of the remark. Even though there are always necessarily facts to come that tend to show any of the possible uses of any phrase to be wrong. But if we deny that the remark is corrigible for reason A we cannot admit that it is corrigible for any other reason there might be for calling it corrigible. Hence we imply that it is not to be called corrigible in that use we are concerned with, because there are facts to come that tend to show it wrong to use the remark in that use. We say that it is not for that reason corrigible although the using of it is linguistically discreditable, for verbal discredit. We say that we cannot have a correction of the statement 'I feel a prick' in the sense we are concerned with, but only a correction of the manner in which the statement is used, when it is used in the way we are concerned with. And this we

refuse to call 'correction of the statement'. And if we did call it "correction of the statement" we might give the idea that the truth of a statement involves logically that there are words in use, namely, the words which are used in the statement and that these words are properly used in the statement. But this idea is madness. For whilst 'I feel a prick' is justifiable and defensible by showing that 'prick' is properly used of such a feeling as I now have, its truth is independent of this. For it is the plain truth that I might feel a prick and nothing ever be said, *a fortiori* properly said. If the verbal defence of a statement were entailed by it, then every statement would assert there are words in use and every statement saying in any way there is no language would be self-contradictory, but this is absurd. Yet at the same time we could and could very well call 'correction' what we now want to call "correction of the manner of use", or "putting into verbal discredit". But what is in this? If there is an interest in this, it must be the unrhretorical interest of Veracity through proper description. For we might say "It is much more proper to call 'I feel a prick' in every of its uses and every other statement 'corrigible' seeing that a correction or discrediting of the use of a statement is much more properly, upon consideration, called 'correction of the statement' than 'not a correction of the statement'. Even going against the assertion 'this is a table' by referring to the furniture register and pointing out its name in the register is "surgical desk" is something one would rather call "correction of the statement 'this is a table'" than "not a correction of the statement 'this is a table'". And this is so in spite of the previously mentioned facts that talk of correction does not readily put in mind verbal correction and that "talk of correction is liable to put in mind correction by the facts". Yet behind the interests of Veracity through proper description there lurk vast rhetorical issues. For a lot depends on attaching a name to the use of a statement after a proof, namely how we affect the current of philosophy with the proof and the name. And we can explain such things as Certainty and Indubitability and Scepticism by giving the proof of incorrigibility of 'I feel a prick' and calling the statement 'Incorrigible'. The name helps remind us of what is in the preceding proof, leads to reflections about the transcendence of knowledge other than knowledge of the psychological present. But can we, having shown how in all humble veracity 'I feel a prick' in all its uses is to be called "corrigible" and how all other statements must be called "corrigible" use this name and proof with favour to explain anything that is a philosophema about the present in particular?

We shall have reminded of a feature that all statements have, of the feature of verbal discredibility, and such a feature will not serve to explain a philosophic doctrine specific to the Psychological present. Which is why in speaking about such doctrines we can't gain by the approach that leads to calling all statements corrigible, and can gain by showing what leads to calling them in-corrigible. The former approach is useful in other places when, *e.g.* we discuss how there is no mystery in knowledge of a universal being exemplified in a particular, *e.g.* how it can be that I know redness belongs to the present visual sense datum. For it then comes to be to the point to say that such things are known as the use of words is, seeing that the question "how can I know that redness belongs to the present visual sense datum" might well be the question "How can I know it is that concept which applies and rightly applies" when this question is not answered satisfactorily by saying "because you see red" but only by saying "because you have learnt a language". What is the problem of such a question is not the Immediacy of the present, but the Remoteness of all judgements including judgements about the present.

If philosophy is concerned with getting clearer why certain things are said by philosophers, if it is in this particular way the pursuit of a useful art *ἀσκήσις τέχνης ἐπιτηδείου*<sup>1</sup> in the words of the Stoics, then it must be admitted that an account of what makes people speak of the certainty of the present is useful. Which I have attempted to give. And it all comes down to this: That what is over now can in no way go on. If the question 'Is this a prick?' is over now, it can in no way go on.

<sup>1</sup> *Plutarch, De plac. phil. 2.*

## V.—THE PHENOMENALIST THEORY OF THE WORLD

BY RICHARD WILLIS

AT one time philosophers tended to construe questions of logic as questions about the world. For example, instead of speaking about statements they spoke about judgements, and treated these judgements as a kind of psychological process. Nowadays the tendency is reversed. All philosophical questions are regarded as logical questions, and philosophers who have asked questions about the world are liable to get irrelevant answers. A banker learning his job might ask "What is the difference between a genuine pound note and a forgery?" To answer, "A forgery can be a convincing imitation; a genuine note cannot", would be to change the subject. The question was about things, not words. The answer was about words—about what predicates can go with what subjects. The philosophical questions, "What is the difference between dreaming and waking? between impressions and ideas?" and many others, are questions about the world just as much as the banker's question. We should be careful to give such questions appropriate answers.

The failure to separate questions about the world from questions of logic has, I believe, obscured some of the most important problems connected with phenomenism. The phenomenist holds two theories, whether or not he realises it: (1) the theory that, in Berkeley's language, there are only ideas, and (2) the theory that sentences about material objects are equivalent to sets of sentences about sense-data. We can call (1) the phenomenist theory of the world, and (2) the theory of reductionism, or phenomenism proper. In modern terminology (1) is the theory that there are only (a) sense-data, (b) entities which are admittedly mental, and (c) sensations, if these are not classed as sense-data.

In calling the first theory a theory of the world I do not mean to say that it is an empirical theory. I think any phenomenist would want to say that it was a logical truth. Logical truths are, perhaps, true in virtue of the meaning of the words in which they are expressed, but they may be *about* anything. "Cats are cats" is about cats; "statements are statements" is about statements. Nor do I wish to deny that existential statements are, in a sense, about properties which are exemplified, but do not occur, in the world. It is, however, admissible to say that the



statement "There are pyramids in Egypt" is about the pyramids in Egypt. In such statements the word for the object said to exist is used and not mentioned. At any rate, the contrast I want to make is, roughly speaking, between "There are only things (in a broad sense of the word) of a certain kind" and "There are only statements of a certain kind".

In order to see more clearly the difference between the two theories, and to understand why they have been confused, let us consider the following sentences and expressions.

- (1) Sentences about material things are equivalent to sets of sentences about sense-data.
- (2) Matter is a permanent possibility of sensation.
- (3) The Constitution of Material Things.
- (4) The relation between sense-data and objects.
- (5) The world does not contain sense-data *and* physical objects in the sense in which it contains chairs *and* tables, or in the sense in which it contains colours *and* sounds.
- (6) There are only ideas.

(1) is a perfectly straightforward statement of the reductionist thesis. (2) is a disguised and vague statement of the same thesis. It appears to be a statement about the world (*cf.* "Matter is a conglomeration of atoms") but is in fact a statement about language. To realise this one has only to consider the meaning of "possibility". (3) is the title of chapter V of *The Foundations of Empirical Knowledge*. In this chapter Professor Ayer discusses (4), which is a relation between sentences, statements, words, *i.e.* a logical relation and not, as might have been expected, a relation between things, like objects, atoms, electrons. In discussing the theory that material things are nothing but collections of actual and possible sense-data, Ayer says: "What is being claimed is simply that propositions which are ordinarily expressed by sentences which refer to material things could also be expressed by sentences which referred exclusively to sense-data." (*The Foundations of Empirical Knowledge*, p. 232.) Such an interpretation is surely the only possible one. (5) is a remark made by Ayer in his paper "Phenomenalism" (*Proc. Arist. Soc.*, 1947-48; reprinted in *Philosophical Essays*). This too looks like a statement about the world, but is in fact about words. Compare "There are not regiments *and* soldiers in the sense in which there are tables *and* chairs." This is a true statement, asserting that the relation of "regiment" to "soldier" is not like the relation of "table" to "chair", since the latter pair of words denote entities which can be separated in a way in which soldiers and regiments logically cannot. If we are ignorant of this fact

we are ignorant of the use of words. It does not, however, commit one to the theory that sentences about regiments are equivalent to sets of sentences about soldiers. Nor does (5) commit one to reductionism. In fact (5) is more or less entailed by (6), the only item on our list which definitely *is* about the world. Compare (6) with: "Before the existence of life there was only matter." (6) looks like this sort of statement and *is* this sort of statement (*i.e.* a ground-floor statement). We must take it at its face value. The language of (2), (3) and (4) is very misleading. People like Ayer are well aware of this fact, and do not misconstrue such language. But the deception works in two ways. If we continually associate with bogus ground-floor statements, the genuine ones will tend to escape our notice.

In discussions of phenomenalism the theory of the world is not usually mentioned. "Phenomenalism" is the name given to the linguistic theory, and nobody seems to bother about the other. Mr. Warnock in his excellent Pelican Book on Berkeley discusses the theory that there are only ideas (pp. 189 ff.), but fails to see its importance. "Why", he asks "should we suppose that, when Locke's 'external bodies' disappear, we really are left only with 'ideas'?" To say that 'there are only ideas' can only be to do one of two things—it is either to assume that, except for the needless excrescence of 'external bodies', Locke's theory is entirely correct; or it is to assert, in a fatally misleading way, that Berkeley's own analysis of statements about material things is right. In neither case does Berkeley's analysis receive any effective *support*. For the doctrine that only ideas exist is either a bad way of stating that analysis itself; or else it is to accept quite uncritically the fragments of another demonstrably vulnerable theory." I think Warnock himself believes that the statement "there are only ideas" is reductionism in disguise. He does not mention the other interpretation in his postscript (p. 245).

But he nowhere shows that the relevant part of Locke's theory is false. He admits that a good sense can be given (even if neither Locke nor Berkeley gave it) to the expression "immediate perception of ideas". The view that we only perceive ideas is revealed as absurd only when coupled with the view that we never perceive the outside world at all. It is admittedly reasonable in normal circumstances to stop using the idea language (if we ever started) and to use the object language instead; and admittedly sentences of the one can never be equivalent to sentences of the other; but these are facts about language, not about the world.

Warnock aptly compares object statements with juries'

verdicts. The verdict is based entirely on the evidence, but is not a summary of that evidence. Similarly object statements are based entirely on the evidence of sense-data, but are not summaries of collections of sense-datum statements. At the same time, in spite of the fundamental logical difference between verdicts and statements of evidence, there are no mysterious, occult facts over and above the evidence which are stated in the verdict. Phenomenalists infer from their theory of the world that verdicts are really summaries of evidence. Warnock, if he regards this as an inference at all, and not as a case of saying the same thing twice in different words, seems to regard it as a valid inference from false premisses to a false conclusion. I regard it as an invalid inference from true, or at least unrefuted, premisses to a false conclusion. To say that verdicts are really summaries of evidence is not to make a statement about the world; it is to give an account of the way in which we talk about the world. The phenomenalist's account of the way we talk is wrong; but to prove him wrong in this respect is not to disprove his theory of the world.

Consider the following parallel. Imagine that Locke and Berkeley, having obtained in heaven a complete list of every human being, past, present and future, together with the date of his death, return to earth and visit Oxford for a philosophical discussion with Warnock. The following dialogue on the subject of general propositions might result.

Locke: "The class of men is an open, not a closed, class. Therefore to say that all men are mortal cannot just be to assert the occurrence of each and every death on our list. There must be some more, totally unknowable deaths over and above those listed here."

Berkeley: "Nonsense! To say that all men are mortal just *is* to assert the occurrence of each and every death on our list. The list is complete, so what else *could* it be?"

Warnock: "No, no! There is a radical logical difference between 'all men are mortal' and 'this man is mortal'. However many statements I assert of the second kind, I shall never get anything that entails a statement of the first kind. You are both wrong. Mr. Locke is talking metaphysical nonsense; Bishop Berkeley is ignoring a plain fact about the logic of ordinary language. As for your list, it is a fatally misleading document forged by the bishop to support his own analysis. In fact it is his analysis in a disguised form."

The moral of this conversation should be obvious.

The fundamental paradox of phenomenalism is the fact that

according to the phenomenalist, when all the world is in a dreamless sleep, nothing exists ; and this paradox is a consequence of the theory of the world just as much as it is a consequence of reductionism. If there are only ideas, when there are no ideas there is nothing. And if we have only refuted the reductionist theory, the paradox remains untouched.

Now it may here be protested that Mr. Berlin has proved this view untenable (" Empirical Propositions and Hypothetical Statements ", *MIND*, 1950, p. 289). He has shown conclusively that categorical statements about material objects are irreducibly categorical ; that the policy of reductionism does not merely happen never to have been successful, but is mistaken in principle. He has emphasised that " such a statement as, ' The table next door is brown ' is existential and as such has existential import, and asserts that something is occurring in a sense in which general or hypothetical propositions proper do not normally assert anything of this sort. . . . One can bring out this point most sharply (at the cost of some exaggeration) by asserting baldly that all theories, hypotheses, general and hypothetical propositions, etc., may be true and yet nothing exist at all ; for if the protases are unfulfilled, the apodoses have no application ; whereas the proposition that some existential material object propositions are true is not compatible with the proposition that nothing exists at all."

I think I agree with nearly everything that Berlin says. In fact I think he might have made even more of the comparison between dispositional and material object statements. The former are irreducibly categorical just as much as the latter (see R. J. Spilsbury, " Dispositions and Phenomenalism ", *MIND*, 1950, p. 339). The evidence supporting the statement " He is clever " will vary from case to case. The man may be a philosopher, mathematician or egyptologist. So no amount of statements containing the evidence (" If you give him tests in mental arithmetic . . . ", etc.) will be the same as the statement they support. Yet in spite of this there is still a radical difference between " Did the world exist even before there was any life ? " and " Is he clever even when he is asleep ? " The contrast therefore is not just the contrast between reducible and irreducible statements. But all this in no way refutes the phenomenalist theory of the world. The theory appears absurd and paradoxical, no doubt ; and since common sense and the logic of ordinary language go hand in hand, this is a relevant argument against reductionism. But the theory I am advocating is a theory about the world, and not about language. Hence Berlin

has at the most *stated* the plain man's objection to it: he has not justified common sense.

The philosopher who first used the word "sense-datum" or its equivalent did not thereby, as is by now well known, discover a new entity in the way Columbus discovered America. By way of reaction to this "discovery" view of the sense-datum it is usual to regard the terminology as a shorthand for what can be said equally well, but at greater length, in everyday English. But this is false. The philosopher in question did discover a new way of speaking. Warnock translates Berkeley's idea language into sentences like "It seems to me as if I were perceiving an orange". This method of translation is, I think, as good as any. But the sense-datum language has a logic of its own. I think it would be incorrect, and not just unusual, to use Warnock's language if there was no doubt at all about the orange; whereas sense-datum statements leave all questions relating to the existence and qualities of objects entirely open. If challenged about an object assertion we should tend to say "*Well*, it seems to me . . ."; and if we are in no doubt at all we should say simply "I can see it". But I return to this point elsewhere. Also there is a difference between "It seems to me as if I were perceiving an orange" and "I am having an orange-like sense-datum" rather similar to the difference between "I am imagining that I am seeing an orange" and "I am visualising an orange". While looking at a stereoscopic picture of the Eiffel Tower I may have a most Eiffel Tower-like sense-datum, but I may also be too acutely aware of un-Parisian noises in the background for it to seem to me as if I were seeing the Eiffel Tower. These differences however, are comparatively trivial. The vitally important fact is that "sense-datum" is a noun designating an entity that can be said to exist or not to exist without reference to percipients. There is no straightforward answer in ordinary language to the question "What is this entity you're talking about?" Hence the illusion of the discovery of some new object. The sentence "There are only ideas" cannot be translated into everyday language, because some ideas are sense-data. And, as far as I can see, this is not just the impossibility of producing an *accurate* translation, but of producing anything at all with the remotest resemblance to the original sentence. Warnock's insistence on translating the idea language, as opposed to merely explaining it, probably accounts for his hopelessly inadequate treatment of the phenomenal theory of the world. We could say: "Apart from sensations and admittedly mental events, it only seems to us ever as if we were perceiving things; nothing else happens or

exists." But this is not only a hideous and nonsensical piece of English ; it implies that we never really perceive anything. A much more plausible attempt at stating the theory is the following: "The fact that things exist is not a fact over and above the fact that it seems to us as if we were perceiving them, in the way in which the existence of chairs is a fact over and above the existence of tables." But what about the fact that in certain unfulfilled circumstances it *would* seem to us as if we were perceiving certain things ? We have, of course, reverted from the world to the realm of language. Instead of talking about the furniture of the universe we have been talking about statements ; for facts just are true statements. We have not even succeeded in translating the sentence "There are not sense-data *and* physical objects in the sense in which there are tables and chairs".

The phenomenalist paradox is not a simple case of using language in a peculiar way. A man who says that a false proposition implies every proposition has to admit, when pressed, that all he means by " $p$  implies  $q$ " is " $\text{not } p \text{ or } q$ ". But the phenomenalist cannot admit that all he means by "nothing exists" is "nothing is perceived, nothing seems to be perceived, and there are no sensations or mental images"; for, unlike the plain man, he believes that once we have said this we have accounted for everything that can happen or exist. Words like "nothing", "something" and "exist" do not vary in meaning ; but they are used (applied) in different ways in different contexts. If a dream exists, it will in some contexts be appropriate to say "something exists", in other contexts it will be appropriate to say "nothing exists"; and so on. Therefore we should not, perhaps, be astonished to find a surprising use of the sentence "nothing exists" in a context where the totally new noun "sense-datum" is being bandied about. The sentence means the same as it always means. No other sentence would serve the phenomenalist's purpose better. But there is no conflict between the "nothing exists" of the phenomenalist and the "something exists" of the ordinary man, any more than there is a conflict between the man who says "There was nothing : I dreamt it" and the man who says "There was something, because you had a dream". To understand how such sentences are being applied, we have to consider their context. Thus the plain man cannot criticise the phenomenalist without philosophising, because the context of the phenomenalist's statement "nothing exists" is philosophical. It is not enough just to consider the ordinary use of words. As long as the philosopher analyses ordinary language, the plain man is in a good position ; for he knows how ordinary language is used.

But if the philosopher starts discussing the world with the aid of technical terms, he has to be met on his own ground.

Perhaps the most hopeful method of attacking the phenomenal theory of the world is to attack the sense-datum theory. It has for some time now been fashionable to assume that the sense-datum is dead. Articles have been written in large numbers to prove that the terminology is a form of nonsense, and scathing references are made to it in discussions of other subjects. However, I remain unconvinced. Most attacks on the sense-datum seem to me to miss the mark very widely. I shall content myself with trying to refute one of the most recent and persuasive attempts to discredit it, namely Mr. Quinton's article "The Problem of Perception" (*MIND*, January, 1955), and then explaining the sense-datum terminology briefly in my own way.

In discussing the various things we may say about the stick, half in, half out of water, Quinton says: "'It looks bent' is the puzzling case. For it may be a guarded way of saying 'it is bent' (denied by 'it isn't bent') or a way of saying 'most people would be inclined to say it was bent' (denied by 'it doesn't') or a way of saying 'it looks bent to me, here, now' (which can only be denied by 'oh surely not')." Quinton would describe the last remark as a statement about one's own experience. He thinks that we are not often aware of our own experiences. ("What", he asks, "of the case of a man lying in the sun on his back with his eyes open and his mind far away? Does he see the blue expanse with shifting white patches on it that he could describe if he were to turn his attention to his visual field?") And this is true even of the case where we are aware of a material object. ("I see a small glassy object in a radio shop and say 'that looks like a valve'. But in fact it is a wineglass. For this error there is no sensory cue; it is the outcome of my general beliefs about the contents of radio shops.") So our experiences, according to Quinton, are the causes of our beliefs about material objects, but not our reasons for such beliefs. Thus if "sense-datum" means the same as "experience" sense-data will not perform the function they were designed to perform. The only alternative is to treat sense-datum assertions as guarded object assertions. But obviously a guarded statement cannot be a reason for an unguarded version of the same statement. Therefore the sense-datum terminology is based on a confusion.

A statement that Quinton neglects when discussing the stick in water is "It presents a bent appearance". Now this has nothing to do with what people are inclined to say. Most adults would in fact, say that the stick was straight. They are used



to such illusions. But the illusion is still there : the stick still presents a bent appearance to everyone with normal eyesight. But if Quinton were to say that " It presents a bent appearance " is about the experience of normal people, then he would have to abandon his belief that we are not in a position to describe our experience unless we are in the " appropriate, sophisticated, phenomenological frame of mind ". However much we examine the stick, however unselfconsciously we peer at it, it still presents a bent appearance. The realistic artist paints the stick, not his experience of it ; but the line on his canvas is bent. The same is true of photographs, although cameras are devoid of experience.

Quinton briefly discusses hallucinations. " We should say of Macbeth ", he remarks, " that he thought he saw the dagger, imagined he could see it, but that he did not actually see it at all ". By " the dagger " Quinton must mean the (non-existent) physical dagger which Macbeth believed he saw. (As a matter of fact Macbeth had no such belief ; at least he was in considerable doubt. We can, however, ignore this Shakespearean detail.) Thus interpreted, Quinton's language is unobjectionable, but he does not mention the fact that Macbeth had a hallucination. That surely was the reason why he believed he was seeing a real dagger. And he did not just have a Quintonian dagger-experience ; for it would be absurd to suggest that if only he had turned his attention to the outside world he would have stopped " seeing " the dagger. One can be frightened by a hallucination, but not by a phenomenological investigation of one's own experience. Moreover one can at the time be quite aware of the fact that one is having a hallucination. Quinton's remarks about Macbeth would not apply to anybody who was not deceived by what he " saw ". Hallucinations may *not* " be regarded as mental images confused with or taken for perceived objects " (R. J. Hirst, *Proc. Arist. Soc.* supp. vol. xxviii, 1954). Mr. Hirst has been justly accused by Mr. Smythies of ignoring the facts about hallucinations (*MIND*, July 1954). But, of course, philosophers need not know such facts provided they have imagination. The argument from illusion would not be affected if there were in fact no illusions. All that matters is that illusions should be logically possible.

Quinton thinks that sometimes we are entitled to make statements about objects although we have no reason for them. " Finally ", he says, " consider standing in broad daylight three feet away from a large and perfectly normal chestnut cart-horse and saying ' that is a horse ' or, more adventurously, ' that horse

is brown'. . . . In these conditions, the challenge 'how can you tell?' is simply devoid of sense."

It may be pointless, in such a case, to ask "How can you tell?" but it is certainly not senseless. One might tell by looking or by hearing the animal stamp or by smell. And one can answer "I can see", "I can hear" or "I can smell". Quinton calls conditions like those he describes in his example "standard conditions". "It will be in such circumstances", he says, "that the use of the sentence will normally be learnt." Now he might object that if some one has his eyes shut and can only smell the horse the conditions, *ipso facto*, will not be standard. But surely this is merely to make one particular reason for uttering a sentence part of the standard conditions for learning its use. This is not to say that there is no reason at all. In the case of the assertion "that horse is brown", it will be even more obvious that the man has found out by looking. But to say that he can tell only by looking is to say that he has only one method of telling, not that he has no method at all.

Quinton might object that "I can see" is *too* good a reason. It logically entails the statement it is intended to support. But this is true of countless other statements and supporting statements. "How do you know there was a needle in that haystack?"—"I found one there." "How can you tell that the government is tottering?"—"I discovered by reading the papers." And so on. But the reason need not entail the statement. Macbeth could at a later date have accounted for his belief about the dagger by saying "I had a hallucination". And the hallucination is not just the cause of the belief but a justification of it. Macbeth's dagger could not have been real: it was floating in the air. But not all hallucinations are like this. If a man not normally subject to hallucinations "sees" a snake in the jungle surely it is *sensible* for him to say "Look out! there's a snake". This sort of case is quite different from, *e.g.* a belief induced by post-hypnotic suggestion. The suggestion causes but does not justify the belief. Moreover we can say when challenged: "Well, it seems to me as if I were seeing a horse." This form of words suggests doubt as to the truth of the original object assertion, but it may still justify the belief even though the belief is implied to be no longer so firmly held. "It seems to me, etc." is not just a guarded material object statement. We can say "It *certainly* seems to me . . .", and "It seems to me as if I were seeing a cat, but there is no cat to be seen". Nor is it a way of expressing an inclination to believe we are perceiving an object: we may use the

words "It seems to me . . ." while denying that we have any such inclination. Also we can surely say "It seems to me . . ." without being in any "sophisticated, phenomenological frame of mind". We may be trying quite unselfconsciously to see some object more clearly. If so, the words are not a Quintonian description of experience.

Suppose, finally, that a blind man could tell by a kind of guess-work what things were around him and what their colours and other characteristics were. Suppose he was invariably right about them. We should say: "He does not guess, he knows". (Compare our knowledge of the position of our limbs. See Wittgenstein, *Philosophical Investigations*, p. 161.) But the only difference between his knowledge and guess-work is the fact that he never doubts or hesitates and is invariably right. Such a man would not have any reasons for his assertions and beliefs about the objects in his vicinity. If we contrast his case with our own it becomes obvious that we do have reasons for such assertions.

I think the best and the simplest way of explaining the term "sense-datum" is to compare it with "hallucination". Both words designate entities which can be said to exist without reference to any one who has them. There may, as a matter of fact, be many types of hallucination which never occur. But it is logically possible to "see" anything that we can be said to see, and the same applies to the other senses. (Even if hallucinations never occurred at all the argument would not be affected except in its power of persuasion.) A hallucination is a kind of sense-datum, but the two words do not mean the same: "I am having a hallucination of x" implies "x does not exist"; "I am having a sense-datum of x" does not have this implication, but leaves the question entirely open whether or not there is anything "public" to be perceived, i.e. objects, mirages, sounds etc. Sense-datum statements thus express no inclination to believe or doubt anything about material things. They are plain, unguarded statements of fact. This definition, as it stands, does not include under the heading "Sense-data", dreams (or rather their component parts) nor sensations, such as toothache. It is intended that with the one proviso mentioned "hallucination" and "sense-datum" should be identical in meaning; and dreams are neither veridical nor hallucinations, and sensations cannot be either veridical or non-veridical. For the purposes of some philosophers such a definition may be inadequate. For my own ends (i.e. making a list of the contents of the universe) it is good enough. Dreams and sensations can be included under "sense-

data " by a process of simple addition, or else they can be added separately to my list of the world's contents.

If I am right, we must either adopt some metaphysical, Lockean theory, or else accept the phenomenalist's account of the universe, however paradoxical it may seem. Thus the alternative " Either Locke or Berkeley " does, after all, represent a choice we have to make. There is no third way. I, for my part, have no hesitation in preferring Berkeley to Locke.

## VI.—DISCUSSIONS

### VINCULA VINDICATA

1. THE picturesque distractions, and studied formlessness, of Mr. Hanson's curious literary style are not the only things that make it difficult to assess the damage that he may have done to the concept of causal chains by his energetic attack upon it.<sup>1</sup> One wonders, is it really the familiar chain analogy that he is attacking, and not some monstrous invention of his own? Surely it is very disconcerting to be told that "the causal chain analogy is appropriate only where genuine causal connexions cannot be expressed,"<sup>2</sup> when there seem to be very obvious reasons, which Hanson constantly overlooks in spite of hovering continually within sight of them, for thinking that the analogy works quite well when such connexions *are* being expressed. Even if the analogy worked only when we had in mind "a series of striking accidents"<sup>3</sup>, we should not for that reason alone have to admit that it had no relevance to genuine causal explanations; for "accidents" themselves have causal explanations, both in science and in daily life, a point that Hanson seems ready enough to concede,<sup>4</sup> evidently without appreciating its significance.

2. Whether there are chain-like processes in nature is a question that we may perhaps postpone,<sup>5</sup> to consider first whether there are not chain-like accounts of causes. I think that it is very easily seen that there are, and that their existence vindicates the chain analogy. For all that can properly be claimed for the chain analogy in its familiar use is that it exhibits the structural features of some very common sorts of causal accounts; but this it does quite successfully; and this is all that it is intended to do, or at least all that can sensibly be attributed in the way of intention to the people who make use of it.

The account of the kingdom lost for want of a nail, which Hanson mentions,<sup>6</sup> has a neat and obvious analogy to chains even in their peculiar feature. The elements of a chain are not only connected with each other; they are in every case *embraced* partly by the previous element and partly by the succeeding one (where both exist). So are the elements of the story. The embracing may be so explicit as to be literally visible; for instance, the old story might be retold in this way:

<sup>1</sup> "Causal Chains", MIND, lxiv, No. 255 (July 1955), 289-311.

<sup>2</sup> Hanson, p. 299.

<sup>3</sup> Hanson, p. 292.

<sup>4</sup> See Hanson, p. 293.

<sup>5</sup> See below, pp. 226-227.

<sup>6</sup> Hanson several times takes as examples for his own purposes causal accounts whose structure the chain analogy seems to have been designed to express, and which it expresses very nicely.

"Because a nail was missing, a horseshoe was lost ;  
 because a horseshoe was lost, a horse was lost ;  
 because a horse was lost, a rider was lost ;  
 because a rider was lost, a battalion was lost ;  
 because a battalion was lost, a battle was lost ;  
 because a battle was lost, a kingdom was lost."

If we identify each *line* as a link, then the fact that it overlaps and embraces the links before and after is visible in the repetition of the last phrase of the line before as the first phrase of the present line, and in the repetition of the last phrase of the present line in the line following.

Alternatively, we might identify the *phrases* that are repeated as the links. Again, the linkage is shown by the repetition. "A rider was lost" is shown connected on the one side with "a horse was lost"; and on the other, with "a battalion was lost". This, I suppose, is the commoner way of applying the analogy, though it leaves half its suggestive power unused. It does not bring out the fact that elements can be found in causal accounts, like this one, which are not only connected in a series, but are, further, connected in something like the distinctive way in which the elements of chains are connected, namely, by overlapping, embracing, and fitting into each other.

In different stories, or in different versions of the same story, the connexions between the elements may be shown with different degrees of explicitness. Only slightly less explicit than the story of the loss of the kingdom in the version just discussed is Papa's explanation to his small son: "You must let me read this book; for if I don't read this book, then I shan't be able to talk about it this evening; and if I'm not able to talk about it this evening, then I won't be paid; and if I'm not paid, then we won't have any money." Here grammar compels slight changes in the phrasing, but this does not obscure the essential fact of repetition, or the linkage, which is still visible.

The traditional version of the loss of the kingdom, in which each line begins "for want of . . .", with phrases like "a rider was lost" echoing in phrases like the "want of a rider", involves slight shifts in conception as well as in mere grammar; but again the essential repetition clearly survives.

A low degree of explicitness, or at least of explicit repetition, can be achieved by using "therefore" to indicate the linkage instead of repeating phrases. Thus, "A nail was missing, therefore a horseshoe was lost; therefore, a horse was lost; therefore, a rider was lost; therefore, a battalion was lost; therefore, a kingdom was lost". A dull way to tell the story, but still a chain-like way; and one that we can make more chain-like at will, expanding it to realize any greater degree of explicitness wanted.

3. In our daily lives, we often have occasion to produce and to take heed of causal accounts that have chain-like features; for we

are often concerned to know the particular successive antecedents of some one given event, or to know the particular successive consequences of some one contemplated action, and chain-like accounts are often the most natural and intelligible way that we have of marshalling the information wanted. So it is with science as well : it would be ridiculous to claim that scientists, acting in their professional capacities, do not often confront particular questions to which chain-like causal accounts are appropriate answers. Whenever they are called upon to specify the antecedents of a single event, it is appropriate that they should supply a chain-like answer.<sup>1</sup>

There is, for instance, the question of the Alaskan caribou. In 1932, the caribou in Alaska numbered 625,000 ; they had dwindled by 1950 to 25,000. How did this disaster come to pass ? The explanation given by F. Fraser Darling, in a broadcast talk on this subject,<sup>2</sup> fell into a general chain-like pattern, the essential features of which survive in the highly abbreviated version that I offer here : " Because of carelessness on the part of gold-rush prospectors, there were widespread fires in the Alaskan tundra. Because of the fires, the lichen cover of the tundra was destroyed. With the removal of the lichen cover, arctic shrubs and herbaceous plants grew up in the tundra region ; and because of this change in vegetation, there was in the winters no longer any food that was suitable for caribou."

In what way is this account, as it stands, less than scientific ? The causal connexions that are mentioned are perfectly genuine ones, which have been corroborated by observation (perhaps by experiment as well) ; and the account is nevertheless chain-like. To be sure, it is a very short account, and the causal connexions mentioned are not very recondite nor very precise quantitatively : but how does this disqualify them ? *Some* genuine causal connexions are of just this sort ; and there are some occasions on which they deserve to be mentioned, even at the expense of other connexions. (The nature of things, the extent of our knowledge, and the brevity demanded in our discourse will determine the occasions.)

Brief accounts can be amplified ; and this one can be amplified in several ways. There might be other causes for the disappearance of the lichen cover besides the fires ; in fact, Darling mentions one—overgrazing by the caribou themselves. Again, if it were desired, the sequence of events could be run through in much greater detail. The fires that destroyed the lichen could, for example, be enumerated separately (at least in principle), and their particular origins specified—an untended campfire here, an overturned lantern there. The action of each fire upon the lichen involved, no doubt, a complex

<sup>1</sup> The event for which an explanation is sought need not be a "breach of routine" (Hanson, p. 309), or anomalous in any way, though I agree that breaches and anomalies provoke more interesting causal questions. The log that is now drifting over the dam is not unexpected ; but it has its own causal history just the same.

<sup>2</sup> See *The Listener* for 5th November 1953.



series of chemical changes, whose temporal sequence could in each case be described. Finally, digressions could be made into general scientific information. Darling, for example, explains that the reason why the moose, which have increased in numbers as the caribou have dwindled, can live upon the shrubs that have replaced the lichen is that they have extraordinarily long digestive tracts.

Do any of these possible sorts of amplification threaten the chain-like structure of the abbreviated account originally given? How could they? The only way that they could do this would be to *discredit* the abbreviated account, forcing us to *discard* it, chain-like structure and all. But only the first point of amplification—the introduction of an additional cause for the destruction of the lichen—remotely suggests that the abbreviated account is mistaken; and this point could easily be accommodated, without making the account un-chain-like. Let the second sentence of the account read: "Because of the fires and because of overgrazing by the caribou themselves, the lichen cover of the tundra was destroyed." There is now an extra link at this point, hooking on, as it were, from the side; but this can happen in real chains—why should it not happen in the analogues? <sup>1</sup>

4. As Hanson understands the chain analogy, the links of the analogue are "discrete events",<sup>2</sup> which are all "similar" to one another "in structure",<sup>3</sup> a property that seems to signify mainly that the events are all "on the same logical level", none of the several words that "express" them being any more "theory-loaded" than the others.<sup>4</sup> But if a chain of events fulfils these conditions, Hanson argues, then the connexions along the chain cannot be causal connexions; for in every causal connexion, cause and effect must be on *different* logical levels, the word or words for the cause being more "theory-loaded" than the word or words for the effect, just as (in Hanson's example) the reference to the wound that caused the scar is more "theory-loaded" than the reference to the scar. Consequently, "the causal chain analogy is appropriate only where genuine causal connexions cannot be expressed".<sup>5</sup>

On the contrary, the links of the lost kingdom adage, the links of Papa's plea to his child, and the links of the caribou story are all causally connected, in a perfectly genuine way; and they are nonetheless similar in structure. Similar, to begin with, in a straightforward sense. The lines may be taken as the links; and the lines are similar in structure: they are all "because" statements

<sup>1</sup> Hanson seems to assume that in every step back in a causal chain we always move from this *single* link to the *single* link immediately previous. See p. 290, where Hanson speaks of "the temptation to think of the course of nature as divisible into discrete happenings each of which has one 'father' or cause, and one, or several, 'sons', or effects". I think that it would be impossible to show that the chain analogy in its familiar use implied anything of the sort.

<sup>2</sup> Hanson, p. 289.

<sup>3</sup> Hanson, p. 290.

<sup>4</sup> Hanson, p. 299.

<sup>5</sup> *Ibid.*

—"because" this happened, this other other thing happened; or they are all hypothetical statements—"if" this happens, "then" this will happen. (There are, of course, many other forms of statement that could be used repeatedly in causal accounts.) If the repeated phrases are identified as the links, these, too, are all similar in structure: in most of the examples given they are all simple subject-predicate sentences; in the caribou story, the repetitive linkage could easily be made more visible by the same device.

So far as the links having to be similar in burden of theory goes, it is not clear that Hanson's doctrine is well-founded; and at any rate it is difficult to see how to apply it. Is the phrase "a horseshoe was lost" more "theory-loaded" than the phrase "a rider was lost"? If it is, then this must not interfere with the phrase "a rider was lost" being more "theory-loaded" in its turn than the phrase "a battalion was lost". The phrase "a rider was lost" does serve doubly in the story, both as something causally explained and as the basis of a causal explanation. And it can do so, of course, only because the context changes: the *explicandum* of one line becomes the *explicans* of the next. But this is just the sort of point that the chain analogy leads one to seize.

It is, then, at least doubtful whether in causal accounts there are *absolute* differences among the elements in respect to being "theory-loaded". But even if there were such differences, why would this prevent the elements of a causal account being joined together in a chain-like way? There might be real chains in which the links differed in *weight* and were arranged in descending order of weight. Yet these links would still be "similar in structure" in all the ways necessary to their being links of a chain.

5. "Man prädiert von der Sache, was in der Darstellungsweise liegt."<sup>1</sup> Perhaps the people who make familiar use of the causal chain analogy are liable to a confusion of this species. But are the errors that the chain analogy leads them into normally serious ones? Who seriously supposes, in philosophy or out of it, that the world, and causal chains spread out in the world, are "held together by a kind of cosmic glue"?<sup>2</sup> Or that causes and effects must be *physically* attached to one another in any way?

To imagine that chain-like causal accounts are justified by something more in nature than the occurrence of processes that lend themselves to chain-like descriptions would require a special effort of fantasy. What motive does a plain man have for such an effort? Hearing of the chain analogy, a man *might* imagine that natural events were like the separate frames of a film-strip, and that each frame showed part of the picture before and part of the picture after; and he might imagine further that everyone could observe these differences and resemblances between the successive frames for himself. But *does* anyone imagine this?

<sup>1</sup> Wittgenstein, *Philosophical Investigations*, p. 46.

<sup>2</sup> Hanson, p. 304.

It is, of course, quite possible that people generally use the chain analogy without having in mind a *distinction* between the properties of certain causal accounts and the characteristic features of natural processes. Hanson would perhaps wish to say that people who fail to make such a distinction are led inevitably to misconceive the nature of science. But, again, the charge fails against the ordinary use of the analogy. Hanson challenges us, jeering, "Approach any research scientist at the Cavendish or Clarendon Laboratories. Ask him how far up the chain he got yesterday. Note the response. Ask him how many causes and how many effects he scored up in his tally book last week." Who on earth has any inclination to ask such questions in the given circumstances? If we sometimes receive chain-like accounts, does it follow that we always expect them? Does the chain analogy, representing accurately the structural features of some sorts of causal accounts, purport to represent the principal activities of scientists at all?

Scientists attempting to reach general theories are not trying to develop chain-like accounts. But the chain analogy in its familiar use does not imply that they do; and let Hanson ask himself what sort of science it would be that would inhibit scientists from giving chain-like causal accounts on occasion.

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## ON THE META-SEMANTICS OF THE PROBLEM OF THE SYNTHETIC *A PRIORI*

THIS note is concerned with the semantics (in a wide sense) involved in discussion of the question: "Are there any synthetic *a priori* truths?" I am not here concerned with the meanings of the truths that are claimed or denied to be examples of the synthetic *a priori*, and least of all am I concerned with the decision as to whether any specific proposition is synthetic and *a priori*. That semantic and other linguistic changes may alter the position of a given sentence in the synthetic-analytic dichotomy is well known; it is likewise granted that the development and progressive articulation of a body of knowledge can alter the attribution of necessity to a given statement. From these now well-known facts it follows that the decision as to whether a given sentence is analytic or synthetic, or a given truth is *a priori* or *a posteriori*, is not the comparatively simple question that it seemed to be when, for instance, Kant could confidently decide that "All bodies are extended" is analytic while "All bodies have weight" is synthetic. In the dispute about specific judgments and the kind of meaning and truth they possess, however, there has been a pervasive change in the meanings of some of the terms *used* in the controversy. Here I shall discuss the changes in the meaning of the terms "analytic" and "synthetic" themselves. Adopting by analogy a somewhat widespread practice in recent discussions, I may be permitted to refer to this discussion as belonging in the "meta-semantics" of the problem of deciding whether any given truth is *a priori* and synthetic.

While the problem of distinguishing what were later to be called analytic judgments from synthetic judgments begins before Kant, these names are his own original technical invention. Unlike the distinction between the *a priori* and the *a posteriori*, which he inherited and modified in few or no essentials while providing a radically new theory about the *a priori*, the distinction between analytic and synthetic judgments is, historically, what he said it was. In his controversy with Eberhard, in fact, he claimed the right of ownership in them, claiming in effect that 'analytic' had to mean what *he* said it meant. No doubt he said different things at different times; the criteria he set up would not be applied in the same way by other thinkers; indeed, he did not himself seem to make unequivocal use of them. But all this can be left aside at the moment, as bearing on points of only historical interest, while we concentrate on one central point he was making throughout his discussions. This essential point was that we can classify judgments in two ways: (a) according to their epistemic status, as necessary or contingent; and (b) according to some aspect of them which belongs to the judgments themselves without respect to their relation to a system of other judgments, the rest of experience, or our sense of assurance

of their truth. The first gives the classification into *a priori* and *a posteriori* judgments; the latter gives the classification of judgments as analytic or synthetic. The former is an epistemological distinction, the latter a logical distinction.

If we recognize that there are two principles of division, we are prevented from solving the problem of whether there are *a priori* synthetic judgments by deduction from the definitions of the four kinds of judgments. No real issue is solved by defining "*a priori*" in such a way that all *a priori* judgments must be analytic. Whether there are such judgments is a question of fact. Undoubtedly the question of fact appeared in a very different light before the invention of non-Euclidean geometries and non-Newtonian physics. Discussion of these facts and not tampering with the definitions has been the fruitful way in which many of Kant's specific decisions concerning the status of single judgments have been settled. But when the question is discussed in greater generality, I think I detect a tendency to modify Kant's definitions in such a way that the general issue is decided against the possibility of *a priori* synthetic judgments. It is this kind of argument which seems to me to be perverse.

I am not here pleading for Kant's definitions, least of all for his definition of "analytic". But I do suggest that any definitions, however drawn, ought to take account of the fact that there is at least a *prima facie* difference, which ought to be reflected in new definitions, between logical necessity and some other kind of necessity which I have called "epistemological". And if the new definitions are to be of Kant's terms "analytic" and "*a priori*", they should take into account the historico-linguistic fact that "analytic" is a logical or a linguistic concept, and that "*a priori*" is an epistemological concept. Any set of definitions or considerations which obliterates this distinction begs the question of whether all necessary truths are logically necessary, and they arouse suspicion of perfectly valid arguments, based on fact, which *might* be given to show that logical necessity is, *in fact*, the only kind of necessity in knowledge.

*Prima facie*, there are four species of propositions or sentences which are abstractly possible: (1) analytic *a priori*, (2) analytic *a posteriori*, (3) synthetic *a priori*, (4) synthetic *a posteriori*. While it might follow from Kant's definitions that the class (2) has no members, that any sentence which is analytic is necessarily true (if true at all), and that a proposition which is *a posteriori* is synthetic, it does not follow from Kant's definitions, or from any other definitions which recognize that two principles of division are abstractly possible, that there are not statements of kind (3).

I shall not recite the details of the history of the long controversy, which began in his own lifetime and which has played so important a part in the development of mathematics, philosophy of science, ethics, and general philosophy since that time. I merely point out that, from about 1920 to about 1950, most competent original thinkers (especially in English-speaking countries) had concluded (3)

to be an empty class, and that at least towards the beginning of the period this conclusion had been based on meticulous examination of the kinds and limits of the necessity claimed for *a priori* knowledge in mathematics, ethics, and science. One of the chief writers on the problem wrote correctly in 1946 that the question of whether there were *a priori* synthetic judgments was "a dead, or nearly dead, issue in philosophy".

It is hard to be confident that discussion of the issue towards the end of the period and since then has always been based on factual considerations of the semantics and syntactics of the truths under discussion. With the triumph of the point of view that there are no synthetic *a priori* truths, there occurred, almost inevitably, a subtle meta-semantic change. Analytic *a priori* was seen as the only species of the *a priori*, and therefore "analytic *a priori*" seemed to be a redundancy. Since it was the analyticity of a statement that rendered it independent of experience, either in origin or in confirmation, the decisive word was "analytic", and "analytic" came to function as a synonym for "necessary", which had previously been regarded as synonymous with "*a priori*". At least they were ascribed the same denotation, and since the criterion of necessity came to be regarded as exclusively logical or linguistic, nothing essential was omitted in saying simply "analytic" instead of "analytic *a priori*". And few people in the main current of recent philosophy talked about "*a priori*" at all.

With the collapse of the two distinctions into one, the debate on the old issue of whether there are any synthetic *a priori* judgments became singularly difficult. For if someone said, "*p* is necessary" *p* would not be granted as a case in point until it was shown that it was logically or linguistically necessary; so *p* came into the discussion only on the condition of its analyticity. And if it could not be shown to be analytic, it was either ruled out as being not necessary, or somewhat vague rules (e.g. a "grammar of colour words") were promised which would show, at some future time, that if necessary it is analytic. Much of this kind of argument was based on the vagueness of terms in *p* that could be fixed only by definitions which would arbitrarily insure the inclusion of the proposition, or its exclusion, as desired. The tactics of such dispute belong to the semantics of the problem, and will not be recounted here. But there was, no doubt, another vagueness which could be fixed by arbitrary definition; this was the equivocality of the word "analytic", and discussion of this does belong to the meta-semantics of the controversy about the status of the disputed truths.

Whenever any term which once had a narrow range of application is made to apply to things which did not fall within its narrow range, if there is not to be an irrevocable obliteration of some important distinctions it is necessary that sub-species be found within the new range of the term. If "analytic" is allowed to take over the entire denotation of "*a priori*" (when the range of "*a priori*" is properly restricted so that it contains all statements thought to be

necessary, by *any* chosen criterion different from the classical or established criteria of logical [or linguistic] necessity), then we may reasonably expect that the sub-species of *a priori*, which were the analytic and the synthetic, will reappear at least transiently as species of the new genus of analytic, the genus I call "analytic<sub>2</sub>". This is reasonable unless we can say that the subdivision of the *a priori* into species, by Kant, had nothing even *prima facie* to recommend it; and it is obvious that there are at least *prima facie* differences (whatever we may think of Kant's treatment) between the necessity or alleged necessity of " $p \cdot q \supset p$ ", "No unmarried man is married", "No bachelor is married", "Nothing red all over is blue", " $7 + 5 = 12$ ", a principle of causation, and " $F = ma$ "—all of which have been called analytic because they are or are thought to be, for various reasons and certainly in various senses, necessary.

We do find what I have said we might reasonably expect to find. Let us take a theory of the analytic which holds that a statement is analytic to the extent or degree to which it will be held impregnable against revision by experience. (Other recent formulations, such as the theory that a proposition is analytic to the extent or degree to which we feel shock when it is denied, would serve equally well, provided only that they do not restrict the range of "analytic" to explicit tautologies—a stringency rare in recent discussions.) Then there are two ways in which a proposition may be found to be analytic (*i.e.* analytic<sub>2</sub>): (1) by inspection of the sentence itself, if it is logically or linguistically true; and (2) by investigation of its role in an organized body of experience we call knowledge. It may well be that we can apply the first test to sentences which are not explicit tautologies, and in this event we have to take a short step to another proposition, such as a law of logic or to a statement in an actual dictionary of standard usage. But even if we do so, we look to rules (or to empirical propositions functioning as rules, as in referring to a dictionary), and we are not likely to become engaged in an arduous search into the actual articulations of an established and internally systematized body of knowledge and precepts of procedure. That there may be some sentences the test of which we do not know whether to describe as (1) or as (2) does not alter the fact that there are some instances when we do apply and know clearly that we are applying one or the other test. The first, which I shall call the "microscopic" test, is *formally* like the Kantian test for analytic (analytic<sub>1</sub>), though it may be vastly improved beyond his somewhat naïve notion that such a judgment is found to be true by an analysis (in a literal sense) of the subject concept. But other statements, such as a principle of causation or even " $F = ma$ ", are found to be analytic<sub>2</sub> by a "macroscopic" test in which we do not inspect the statement itself or analyse its subject concept, but analyse (in some non-literal sense<sup>1</sup>) the context of

<sup>1</sup> On the literal and non-literal meanings of "analysis", and the consequent confusion in the meanings of "analytic", see the acute remark of R. Robinson, *Definition*, pp. 188-189.



organized knowledge of which it may be an essential structural member. Now by this test statements may well turn out to be analytic<sub>2</sub>, i.e. necessary in some sense, which need not be analytic<sub>1</sub>.

But what is "analytic<sub>2</sub>"? It is not defined by the procedure which defined "analytic<sub>1</sub>", but simply by reference to the statements' necessity (in an epistemological or methodological, and no doubt watered-down, sense which would not perhaps in some cases have been recognized by Kant as constituting necessity at all). Here "analytic<sub>2</sub>" means necessary in *some* sense, and not exclusively in the logical or microscopic sense ("analytic<sub>1</sub>"). But then "analytic<sub>2</sub>" is formally equivalent to the old "*a priori*". Or, better, it is a substitute for the old "*a priori*", differing from it, if at all, only in the somewhat more modest claims made for the exclusiveness and irrevocability of the decision that a given statement is analytic<sub>2</sub> but not analytic<sub>1</sub>—a modesty and tentativeness singularly absent in most earlier decisions about whether a statement was a *a priori*.

Here we observe the meta-semantic shift to which I wish to call attention. "Analytic" comes to mean the same as "*a priori*" with all the vagueness or precision of the latter. But this meaning of "analytic" is not the classical meaning, and is indicated as "analytic<sub>2</sub>". Then there are under it two species, the microscopic and the macroscopic analytic. The former is the logical or linguistic analytic (analytic<sub>1</sub>), and the latter means any one or more of the following: "analytic truths which are factual or 'real'", the "functionally *a priori*", the "methodological *a priori*", the "hypothetically necessary", the "material *a priori*", the "regulative *a priori*",—and the "synthetic *a priori*".

Now just the same reason that one might now say that a proposition like a principle of causation is (macroscopically) analytic in a particular language or body of science led Kant to say that it is a *a priori* synthetic, viz. that it makes experience of objects possible, and is not known to be true either by reference to empirical facts or by inspection of its logical form but by reference to its contribution to the "possibility of experience". In fact, I suspect not only that there are the same reasons for saying both; I suspect also that, apart from the more modest claims made for any specific statement claimed to be analytic<sub>2</sub> (and indeed analytic<sub>1</sub>), the same thing is being said. *Plus ça change*. . . .

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## TIME AND CHANGE

### I

WHENEVER we make attempts to explain the nature of time we seem forced to make use of metaphors, generally of a spatial character which lead to more puzzles and confusion than any that might have occasioned the attempt to explain. This fact has recently been brought afresh to our attention by Professor J. J. C. Smart in an extremely able and interesting article which he concludes by recommending that we should do best 'not to shift our syntax . . . to avoid the temptation to spatialize time or to hypostatize events'.<sup>1</sup> No doubt the advice is good, but is it possible to follow it? If we do follow it how are we to talk about time? If we accept Mr. Hartland Swann's<sup>2</sup> suggestion that all puzzles may be avoided by confining ourselves to statements involving only the 'before-and-after relation', we shall have to refrain from any usages other than 'A is before B', 'B is after A', 'X is simultaneous with Y'. This may do well enough, but it restricts our discourse very severely and does not altogether succeed in avoiding that shift or mixture of syntax against which we have rightly been warned. We shall not altogether have avoided spatialization of time. The relations of 'being before' and 'being after' can also be spatial relations (synonymous with 'in front of' and 'behind') and they follow very similar rules of syntax whether used of spatial or of temporal facts especially when movement is involved. Professor Smart has warned us that it does not help to point out that in the case of time they are 'irreversible', for that is a term strictly applicable only to movement and is itself a spatialization. Likewise 'simultaneous', though it cannot so easily be spatialized, inasmuch as it is a word reserved for time, is very like 'coincident', which can often be substituted for it. Speaking thus of events encourages us to think of them as if they were arranged along a line or queue and it is a short step to thinking of time as a fourth dimension, a notorious form of spatialization.<sup>3</sup> And if we speak only a little more loosely

<sup>1</sup> 'The River of Time', *MIND*, lviii. 494.

<sup>2</sup> See 'The Concept of Time', *Philosophical Quarterly*, v. No. 18 (January 1955), 18.

<sup>3</sup> In a further discussion in *MIND* (April, 1955), Professor Smart distinguishes two senses in which one may be said to spatialize time, and says that in one of these 'it is a thoroughly laudable thing to do'. This laudable form of spatialization is the use of four-dimensional geometry interpreted as a geometry of space-time. The representation of things and events in terms of such a geometry, Smart points out, excludes change and movement, so that it becomes illegitimate to 'think of time as an extended something along which we can move'. But in that case, time is not spatialized so much as eliminated altogether. Such a method of

(or would it, in fact, be a loosening of usage—and, if so, of what usage?), and say 'A comes before B', 'B comes after A', or better 'A came before B' and 'B came (or will come) after A', are we not using spatial metaphors? Do events 'come'?—*Can* they 'come'? Is it not the same to say that they 'come' as to say that they 'travel' or 'move nearer'? And are we not immediately committed to spatial imagery? I have argued elsewhere<sup>1</sup> that time-puzzles are not soluble, as Professor Findlay has suggested, by adjustments of linguistic usage, yet, as both he and Professor Smart have fully demonstrated, they are very real and very perplexing. Verbal habits and expressions are certainly closely connected with them, but the difficulties are essentially difficulties in making clear to ourselves just what time and temporal relations are; just how we can conceive time, other than confusedly.

At the cost of repeating what others have said before, I should like to consider briefly some of the pitfalls that beset the path of any investigation into the nature of time or the puzzles it engenders. It seems as if we *cannot* speak of time or temporal experiences without the use of some spatial metaphor. To become involved in this difficulty we do not need to go to the lengths of calling time 'an ever-rolling stream', nor even to go so far as to entertain the similar notion of a kind of conveyor belt, revealed by Hartland Swann as the tacit assumption of some popular scientific writings.<sup>2</sup> Our language abounds with similar images: 'the march of time', 'the flow of time', 'the course of time', 'the passage of time', are all spatial metaphors carrying the same implication—that of some mysterious kind of fluid (or army) that 'passes'. 'Passes what?' we may ask. The absurdity of this idea is emphasized when we speak of time passing slowly or quickly, for how can we measure the pace of time? The measure of speed can be in terms only of distance in space, covered in a certain period of time taken as a unit. Speed, therefore, implies space as well as time, so that any talk of time as a movement (flow, passage, or what you will) or of the pace at which it passes is not only an example of spatialization but also a descent into an infinite regress, because a presumed *movement* of time requires another time in which to move. Or, to put it slightly differently, if we allege that time passes quickly we should be able to ask how quickly the last five minutes has passed. How long did they take to elapse and how much less or more (time!) than the previous five

representing space-time is acceptable only as a conceptual framework for mathematical reasoning. If we regard it as a representation of the world, and attempt to interpret it in terms of our common experience, we are liable to think of the time axis as a line along which we move as we successively experience events. Professor Smart recognizes this when he objects to the talk of light signals being transmitted from one point of Minkowski space to another.

<sup>1</sup> 'Misleading Analyses', *The Philosophical Quarterly*, iii. 295-297.

<sup>2</sup> See *op. cit.* p. 11.

minutes? This way of thinking and speaking is palpably absurd, involving the assumption of an ulterior time series, against which the 'passage' of the normal time series is to be measured. That ulterior time series will then also be a 'passage' and the infinite regress is evident.

Yet how do we avoid it? Hartland Swann has shown that to speak of 'time intervals' is to spatialize just as it is to speak of 'time flow' or its rapidity. The idea of an interval is primarily spatial and it presupposes a line joining the extremities between which the interval falls. The interval between events (greater or smaller) is recognizable only in terms of a measure. If we deny this and contend that it is possible to be aware of a time interval between two events without measuring it in any precise way, we simply return, in effect, to the before-and-after terminology. For the sort of awareness referred to is simply the awareness that one event occurred some time before the other and if we describe this as an interval we can hardly escape the spatial imagery of linear order and distance. But pure time cannot be measured. When we claim to measure a time interval we are always measuring only a space interval, which we (arbitrarily) correlate with it—the distance traversed by the hand of a clock, or the shadow of a gnomon, or the like. The interval is arbitrary because it depends on the speed of the movement, which is fortuitous. This distance traversed is supposed to mark off the time interval. But again we have not only spatialization but also infinite regress; for the space interval measures the time only if it is traversed in a definitely determined time. The movement of the clock-hand can measure time only if it traverses the prescribed distance in a prescribed time, so that this prescribed time demands another measure, or method of measurement, to determine the acceptability as a measure, of the movement of the clock-hand. To repeat in a slightly different way, the measure of the time is the distance traversed, at a prescribed speed, in the time to be measured; but the measure of the speed is the time taken to traverse the distance, and this time should, but cannot, be measured independently. Consequently, time cannot be measured at all, for no 'section' of the 'time-flow' (again spatial metaphors) can be directly compared with, or directly applied to any other section; and there can be no other time series against which it can be measured (as the length of a piece of string can be measured against the length of a yard-stick). But no more can a lapse of time be directly compared with or applied to a distance in space. There can thus be no measure of time.

Yet, it will be countered, we do measure time and our measurement is practically useful both for the ordinary affairs of life and for the more precise needs of science. What is it then that we measure? and how do we measure it?

## II

It seems that, however we think and speak of time, to whatever linguistic device we resort, we cannot avoid thinking in terms of movement. We think of time either as a static line along which, or surface over which, or, again, as a medium through which we move;<sup>1</sup> or we think of it as a stream, or belt, or column which moves past us. And the oddity of the image is high-lighted when we notice that, in the first case, our 'movement' through (or along, or over) time continues even when we do not move in space at all; and, in the second case, time is thought to move past us irrespective of our own rest or movement in space. In short, the image is sheer spatialization, treating time as a fourth dimension, added to the other three, so that the required movement in that dimension can occur independently of movement in any of the others, the only proviso being that, in the time dimension alone, rest is impossible and the movement is irreversible. Any such form of imagery, however, leads to absurdity because movement, of necessity, implies *both* space and time, and in the image, while time is represented by a spatial factor, the no less essential temporal factor is overlooked. Yet if the image is to serve at all this ulterior temporal factor has to be presumed and we are back where we started. Movement along a fourth dimension still implies a time which is none of the four dimensions, but demands, if the same treatment is to be applied to it, afresh, a fifth dimension, which would in turn bring no remedy.

Is the source of our difficulties merely that we think in pictures where pictorial thinking is inappropriate? Undoubtedly, the pictures that we use are inappropriate, but that cannot be the whole of the trouble because the pictures seem almost to be forced upon us. It is not simply a matter of imagination, for when we imagine we simply reproduce mentally what we have perceived. If I imagine a horse, I repeat in mental imagery what I perceive when I see a horse; if I imagine a gryphon, I combine in mental imagery part of what I perceive when I see an eagle with part of what I perceive when I see a lion, and so on. But when I imagine a stream, or an arcade ('the arches of the years'), or a ship sailing over a sea, in my effort to conceive the nature of time, I am not just repeating in imagination what I perceive; for these are mere metaphors and I do not actually perceive any of them in being aware of time, as such. In actuality I do not perceive time, as such, at all. What I perceive is change; and it is failure to observe this distinction, which vitiates much that is written about 'our direct experience of time' and the so-called 'specious present'.<sup>2</sup> But I shall not concern myself with that matter here.

<sup>1</sup> Cf. J. J. C. Smart, *loc. cit.*

<sup>2</sup> Cf. J. D. Mabbott, 'Our direct Experience of Time', *MIND*. vol. lx; and C.W. K. Mundle 'How Specious is the Specious Present?', *MIND*. vol. lxiii.

If we could renounce all pictures and all metaphors, what then should we do? The current answer of philosophers is that we should keep a guard upon our tongues and talk of time only in certain ways, regulate our syntax carefully and confine ourselves to certain restricted forms of locution. But we have seen that these remedies do not serve. No form of locution wholly evades the difficulties and the accepted syntax of time expressions is fatally similar to that of space expressions—so similar, in fact, that it cannot make the essential difference between time and space intelligible. The trouble lies deeper than language, in what we are trying to express by our words. Metaphorical language is the expression of pictorial thinking. If we are to abjure this and if we are to speak so as to avoid absurdities, we must think in some other way. We can determine the right way to speak about time, only when we have decided what is the right way to think about it. Our efforts at pictorial thinking are prompted, in this case, by the desire to understand, and we can satisfy that desire only by careful examination and analysis of the experience of change and succession.

### III

The reason for our seeming inability to represent time otherwise than in the obviously illegitimate way, as some form of motion, is that time is neither possible nor conceivable without change. This dependence of time, both in fact and in conception, upon change, was demonstrated by John Macmurray in an Aristotelian Society symposium in 1928 and those who have disputed and denied it have done so misguidedly.<sup>1</sup> It has been maintained that duration is possible and involves temporal lapse, even where there is no change whatever. It is often argued that, even if time could not be *experienced* without change, a universe without change could nevertheless exist and endure despite our inability to experience it.<sup>2</sup> But this is surely false. Duration involves, and must involve, the distinction, within the period of duration, of before and after, earlier and later. This again implies the possibility of discrimination between the events that are so related. But if no change is to occur no such discrimination is possible; and if no relata can be distinguished no relation between them can be discerned. Nor is this only a subjective matter of our capacity, for the view under discussion posits a reality without change, which would be either a reality without events, or (what comes to the same thing) one in which all events are exactly alike—i.e. indistinguishable. If there are no events, there are no temporal relations between events. If all events are exactly alike, there is no possibility of dating them,

<sup>1</sup> See 'Time and Change', Symposium by John Macmurray, R. B. Braithwaite and C. D. Broad, *Proceedings of the Aristotelian Society*, sup. vol. viii (1928).

<sup>2</sup> Cf. R. B. Braithwaite, *loc. cit.*, p. 162, and Hartland Swann, *op. cit.* p. 7.

because all dates would similarly be exactly alike or indistinguishable. The only possible discrimination between the parts of time is the discrimination from one another of events, and if events are indistinguishable, the parts of time must be likewise. But to say this is to say that there is no such thing as time—no ordered series of events related as earlier, simultaneous, and later. It might be argued that though such events may be psychologically indistinguishable they are nevertheless logically distinguishable. They are all exactly alike, but are numerically different and can be identified as 'the first', 'the second', 'the third', and so on. But this is to beg the question. The distinction between the first and the second here postulated is a disguised temporal distinction. It presupposes that one comes before another, that the first is earlier than the second and so on. But what is the distinction between the earlier and the later? It cannot be constituted by counting, for that presumes that it already exists and can be determined in some other way—i.e. that there are discriminable units to count. Moreover, counting is itself a series of changes and to make duration dependent upon the correlation of an unchanging 'series' (if the phrase is not a contradiction in terms) with the series of natural integers, is to give up the position that duration is possible *without* change.

Writers seem to have been misled into maintaining this position by an oversight very easy to make. It is thought that what does not change might very well endure because we are at times aware of objects which persist unchanged for some time. A sustained note on a violin has been given by Hartland Swann as one example, and there are many others. What is overlooked, however, is the fact that the duration of an unchanging existent is wholly dependent upon its correlation with some series of contemporaneous changes. In the case of the sustained note on the violin, its duration is only expressible in terms of some series of other events involving change. It may be any or all of the following: concurrent awareness of the fading memory of the preceding sounds or silence, the concurrent sensation of mounting nervous tension, concurrent movements of the violinist's hand in drawing the bow across the string, and so on. If we raise and try to answer the question, How long did it last (or has it lasted)? we can do so only in terms of some such series of changes (which is precisely the case when we give a measure of time, like ten seconds; the series of changes now being supplied by the movement of the hand of the stop-watch). The error of overlooking this fact is common to all arguments in which it is maintained that duration is possible without change.

It might be held, however, that the fact that we can measure duration only in terms of concomitant changes does not prevent things from persisting unchanged for a longer or shorter period of time. Rather the facts that things do and are known so to persist and that we can measure their duration by reference to concomitant changes, are evidence that persistence without change is possible. My argument, however, is not that nothing can endure or per-



sist, but that duration as a form of temporal existence always implies change. I should be prepared to go further and to argue, with Kant, that permanence is possible only for the changing, and I believe that Kant's position is the basis for my case. But it is not necessary for my present purpose to argue in this way. It is sufficient to show that all temporal relations, either in an enduring or in a changing object, depend on variation, either directly or through correlation of the persistent with the changing.

## IV

Change may be of three different kinds. It may be qualitative, quantitative or positional, and of these we may neglect quantitative change as reducible either to qualitative (change of intensive magnitude) or to positional (change of extensive magnitude). As the notion of time derives from that of change, it will be conceived either in terms of qualitative change or of positional change (movement). But all change is change in relations: qualitative change is that of the qualitative relations between qualified subjects. If a thing changes in colour it changes its relation to other coloured things in this respect. Changes in the quality of sound constitute a succession of different sounds variously related in pitch, intensity or timbre. The same is true of other qualitative changes, as well as of positional change, which is change in spatial relations. These are the only possible varieties of change, for that of temporal relations (as Smart has shown)<sup>1</sup> is strictly impossible. What comes first in a temporal series, is always first. It cannot change its temporal relations to what comes after it. And so it is with all 'positions in time'. They cannot be altered. It is this unchangeableness in temporal relations that we have in mind when we say that time is 'irreversible'. In other words, *there can be no temporal change*. But while events cannot change their temporal relations with respect to one another, they are themselves changes and are constituted by change in other kinds of relations between things. Time, therefore, is not itself a movement, or series of changes, at all, but a scheme or order of unchanging relationships, which we impose upon the series of changes in relations other than temporal. It is like the marks on the ruler, which remain always the same with respect to one another but enable us to determine the spatial relations (and their changes) between things in space. Now space is also an order of relations and (as in the case of the marks on the ruler) this order does not itself change, though things, in moving through space, change their position—that is, assume different spatial relations with respect to each other. This does not happen in the case of events in time. Their temporal position is fixed and unchangeable.

Where a thing moves in space, we can compare its new position with its old because the old position is still there available for our

<sup>1</sup> MIND, lviii. 490 ff.

inspection and comparison ; and for the same reason, measurement of length presents no difficulty. But the 'positions' of events in the temporal series cannot be compared in this way, because only one such position is available to us for inspection. Past events are no longer with us and future events are not yet. All qualitative change is subject to this difficulty. Only the present quality is inspectable. That from which it has changed and that into which it will change obviously cannot be simultaneously available for comparison. Consequently, in our effort to think of the succession, we revive the past in memory and anticipate the future in imagination, relating them to one another as we would positions along a line in space, and we think of the change from one to the other as movement along the line. Qualitative change is thus assimilated to movement, and, when we try to present to ourselves the nature of time, being unable to do so without some reference to change, we illegitimately make use of change in position (or movement) to represent what essentially is the unmoving and unchanging correlate of all change. This unchanging correlate, as Kant taught us long ago, is Time.<sup>1</sup>

## V

We may now return to the questions at the end of the first section. When we think we measure time, as we commonly do, what is it that we measure ? and how do we measure it ? The answer to the first question is : the rate of change. We measure the rate of one process of change in terms of another, but the measuring scale that we use, like all measuring scales, is a scheme or set of fixed events in regular, unchanging, mutual relationship. This fixed scale is time, and we do not measure it any more than, when we measure the length of a beam, it is the measuring rod that is being measured. But, whereas the divisions of the linear scale are marked off by points, the divisions of the temporal scale are marked off by events, and for this purpose events are sought, the temporal relations between which are as equal and regular as we know, *e.g.* the successive completion of its revolution by the earth in its orbit, or of its rotation on its axis. These are the events which Hartland Swann calls 'yardstick-setters'.<sup>2</sup> The units of measurement are, of course, arbitrary ; that is to say, chosen for convenience according to the processes that we wish to measure, but this is true of all units and all scales of measurement.

If it is accepted that space and time are arbitrarily chosen systems of unchanging relations imposed upon the flux of events as we experience them, it seems to follow that they are not elements in the nature of things—not real—but a sort of mould into which, by our thinking, we pour the molten flux of our experience. They are ways in which we organize the confused variety of immediate experience to constitute a systematically related world of objects.

<sup>1</sup> *Critique of Pure Reason*, A. 182-3, B. 224-5.

<sup>2</sup> *Op. cit.*

This, again, is a Kantian doctrine, and it has an unwelcome air of subjectivity. Are we now to say that really there is no spatio-temporal world but only a flux (a Kantian manifold) of sensuous experience out of which we construct such a world in our minds?

There is no need to draw this conclusion or to acquiesce in any subjectivist view. What our thought imposes upon the flux of sense-experience is the metric by which we determine more or less precisely the relations between events. This, it is true, is arbitrary and may be regarded as subjective. But the flux itself is part of the continuous process of change which is the real world. I cannot enter fully at this point into the question of the relation between sense-experience and the physico-biological world, knowledge of which we derive from it; but I should adopt a position similar to Whitehead's, that the process of sense-experience is a late phase in the wider process which is the real substance of the world. That the former brings the latter to a higher degree of organic unity or integration and so nearer to fruition. The knowledge of the world which develops consequently is a still more complete fruition, a still more explicit and comprehensive integration, not a replication or representation of an original model. The process of change, therefore, is real enough even though the metric is arbitrary, which we impose upon it in our efforts to specify and determine exactly certain of the relations between its parts, elements and phases. Space and time are, in short, what Whitehead calls 'co-ordinate division of the concrete',<sup>1</sup> and their ideal character is no different from and need surprise us no more than, the arbitrary choice of units for the measurement of length and duration which men have from time to time adopted.

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<sup>1</sup> *Process and Reality*, pt. iv, ch. i.

## INFINITY

THIS is a note on the definition which Bertrand Russell gives of the mathematical concept of Infinity in his Essay "Mathematics and the Metaphysicians" which appears in *Mysticism and Logic* (recently republished in Pelican Books). This essay was first published in 1901, but the views which are there expressed about Infinity have not, as far as I know, been revised by the author later, although he puts them in more mathematical language in his *Introduction to Mathematical Philosophy* (1919).

It is necessary to quote at some length :

The philosophy of the infinite, on the other hand, is wholly positive. It was formerly supposed that infinite numbers, and the mathematical infinite generally, were self-contradictory. But as it was obvious that there were infinities—for example, the number of numbers—the contradictions of infinity seemed unavoidable, and philosophy seemed to have wandered into a 'cul-de-sac'. This difficulty led to Kant's antinomies, and hence, more or less indirectly, to much of Hegel's dialectic method. Almost all current philosophy is upset by the fact (of which very few philosophers are as yet aware) that all the ancient and respectable contradictions in the notion of the infinite have been once for all disposed of. The method by which this has been done is most interesting and instructive. In the first place, though people had talked glibly about infinity ever since the beginnings of Greek thought, nobody had ever thought of asking, What is infinity? If any philosopher had been asked for a definition of infinity, he might have produced some unintelligible rignmarole, but he would certainly not have been able to give a definition that had any meaning at all. Twenty years ago, roughly speaking, Dedekind and Cantor asked this question, and, what is more remarkable, they answered it. They found, that is to say, a perfectly precise definition of an infinite number or an infinite collection of things. This was the first and perhaps the greatest step. It then remained to examine the supposed contradictions in this notion. Here Cantor proceeded in the only proper way. He took pairs of contradictory propositions, in which both sides of the contradiction would be usually regarded as demonstrable, and he strictly examined the supposed proofs. He found that all proofs adverse to infinity involved a certain principle, at first sight obviously true, but destructive, in its consequences, of almost all mathematics. The proofs favourable to infinity, on the other hand, involved no principle that had evil consequences. It thus appeared that common sense had allowed itself to be taken in by a specious maxim, and that, when once this maxim was rejected, all went well.

The maxim in question is, that if one collection is part of another, the one which is a part has fewer terms than the one of which it is a part. This maxim is true of finite numbers. For example, Englishmen are only some among Europeans, and there are fewer Englishmen than Europeans. But when we come to infinite numbers,

this is no longer true. This breakdown of the maxim gives us the precise definition of infinity. A collection of terms is infinite when it contains as parts other collections which have just as many terms as it has. If you can take away some of the terms of a collection, without diminishing the number of terms, then there are an infinite number of terms in the collection. For example, there are just as many even numbers as there are numbers altogether, since every number can be doubled. This may be seen by putting odd and even numbers together in one row, and even numbers alone in a row below :

1, 2, 3, 4, 5 *ad infinitum*.

2, 4, 6, 8, 10, *ad infinitum*.

There are obviously just as many numbers in the row below as in the row above, because there is one below for each one above. This property, which was formerly thought to be a contradiction, is now transformed into a harmless definition of infinity, and shows, in the above case, that the number of finite numbers is infinite.

It will be convenient to begin by considering the proof which, it is claimed, demonstrates that there are just as many even numbers as there are numbers altogether.

The number of numbers is indefinite, unlimited ; however big a number you think of I can always cap it by adding 1, or multiplying by 3, or by 1765, or by any other integer. It is clear to anyone, as soon as they understand what is meant by a number and how the process of counting is carried out, that it is a process which can go on without limit. This 'proof' demonstrates that if you start to count out loud 1, 2, 3, etc., I can always reply with an even number for every number you say. We can both go on indefinitely without repeating ourselves and I shall never be at a loss for a reply : the 'contest' between us will be level pegging the whole way. "Therefore" there are as many even numbers as there are numbers.

But supposing the contest is arranged differently. You call out the even numbers in order—2, 4, 6, 8, etc., and I reply by repeating them and then saying the preceding odd number. I will therefore say 2 and 1, 4 and 3, 6 and 5 . . . etc. It is clear again that we can both go on indefinitely without repeating ourselves : I shall never be at a loss for a reply and I will have produced twice as many numbers as you have.

"Therefore" there are twice as many numbers as there are even numbers.

Or, again, suppose the contest is arranged in yet another way. You call out the even numbers in order, 2, 4, 6, 8, etc., and I reply by doubling them and then saying the three preceding numbers. When you say 2, I will say, 4, 1, 2, 3 ; when you say 4, I will say 8, 5, 6, 7 and so on. It is clear that we can both go on indefinitely without repeating ourselves : you will be saying all the even numbers, and I will be saying all the numbers. But I will say four times as many numbers as you do.

"Therefore" there are four times as many numbers as there are even numbers.

There seems no reason to maintain that any one of these "proofs" is more valid than the other two. Are we then to say that they are all valid or that none of them is?

The fact surely is that once we start to talk about an *indefinite* number the concepts "as many as", "equal to", "twice as many as", are simply not applicable.

A number may be indefinite in more senses than one. We may say that the number of people coming to the party this afternoon is "indefinite" because we just do not know how many there will be: we issued a general invitation and we have as yet no means of telling (perhaps it is not yet decided) how many will make up their minds to accept. This is to use "indefinite" in the sense of "not known and, perhaps, not decided".

Or we might say that the number of absent-minded professors in England is indefinite. In this case it is indefinite partly because it is not known, but also, more importantly, because it is not defined what it is to be absent-minded. But it is perfectly possible in theory, though it would be rather artificial, to define "absent-minded" in a clear-cut way and to make whatever investigations are necessary to enable us to answer precisely the question "How many absent-minded professors are there in England?"

But the number of numbers is indefinite in quite another sense. It is not a question of there being an answer only we do not know what it is, nor is it a question of its being indefinite because of a lack of definition of what we mean by the number of numbers. It is without limit, as big as we please. And if we are comparing two collections, both of which are without limit or have no definite size, it is nonsensical to start comparing their sizes by talking about "as many as", "twice as many as", and so forth. As soon as we understand in what sense the number of numbers is indefinite we see that the question "How many?" is not just unanswerable but inapplicable. It is like arguing about which is the redder of two colourless gases.

But, it may be argued, the words "as many as", "equal to" and so on are being employed by Russell in a rather unusual way. Mathematicians are in the habit of doing this, they are entitled to do it and it is very naïve to expect the employment of these simple words by professional mathematicians to be in accordance with the usages of the plain man. The plain man is conditioned, it might be said, to use these phrases in certain ways when applying them to definite numbers, he must not expect them to mean the same thing or to be used in the same way when they are applied to indefinite numbers.

There are four points that might reasonably be made in answer to this:

(1) It is customary to give notice if a word is going to be employed in an unusual sense and to say what that sense is.

(2) It is not legitimate to introduce this new sense for the first time in a definition of something else. Russell is defining Infinity in terms of the concept "as many as", among others. The definition would seem to lose its value if he then turns round and explains that of course "as many as" was not being used in its ordinary sense. Surely one foot must be kept on the ground.

(3) In the passages quoted Russell certainly does not appear to be introducing new meanings for "as many as" and allied phrases. What he seems to be doing is to be telling us something about Infinity in terms of concepts with which he supposes us to be already familiar. He does not appear to be taking it for granted that we know what Infinity is and drawing our attention to new meanings he is attaching to such phrases as "twice as many as".

(4) If a word is being used in a new sense it is not legitimate to achieve a paradoxical effect because of its associations with the old sense. If I decide to use the verb "to fly" to denote what is ordinarily meant by "to walk", and if I say "My pigs can fly", I am not entitled to expect a chorus of admiration and wonder at the skill with which I have taught my pets to solve the problems of levitation.

This essay of Russell's was to a large extent designed to be popular. It was written for an American magazine, *The International Monthly*, and as Russell remarks in the Preface to *Mysticism and Logic*: "its tone is partly explained by the fact that the editor begged me to make the article as romantic as possible."

Let us then see how the same ideas appear in more formal mathematical language.

In his *Introduction to Mathematical Philosophy*, Russell says (p. 18):

Whatever number of terms a collection may have those collections that are 'similar' to it will have the same number of terms. We may take this as a *definition* of 'having the same number of terms'.

Earlier (p. 16) he has defined two classes as 'similar' when there is a one-to-one correspondence between their terms (he calls it a one-one relation) and this correspondence is formally defined thus:

A relation is said to be 'one-one' when, if  $x$  has the relation in question to  $y$ , no other term  $x'$  has the same relation to  $y$ , and  $x$  does not have the same relation to any term  $y'$  other than  $y$  (*op. cit.* p. 15).

Finding out whether there is a one-one relation between two classes is a method, says Russell, of deciding whether they have the same number of terms.

It should be noticed that as applied to finite classes there are two different kinds of example.

There is a one-to-one correspondence between the heads on proper pennies and the tails on proper pennies. We can therefore say that the number of such heads is the same as the number of such



tails. We can say this with certainty, without knowing what the number is, and we can also say that it is a definite number even though we are unlikely ever to know what definite number.

The number of terms in these two classes is the same *because* there is a one-to-one correspondence between them.

Now consider two classes which might fortuitously happen to have the same number of terms—for example, the Fifth Form at St. Dominic's and the Sixth Form at St. Trinian's. If we want to find out whether they are the same and we are unable to count—or are forbidden to do so by the rules of some mathematical game which we happen to be playing—we can pair them off. And if we find that there is no-one left over when the pairing is completed we can say that the two classes have the same number of terms or, in a sense, as they are paired off one-to-one, that there is a one-to-one correspondence between them.

But this one-to-one correspondence is of a rather different kind from that which obtained between the heads and the tails. In this case it seems that we can say there is a one-to-one correspondence *because* they are the same rather than the other way round. In fact we shall have to strain Russell's original definition somewhat before we can admit that it is proper to apply the phrase to a case of this kind. The pairing was entirely arbitrary and  $x$  might just as well have been paired off with  $y'$  as with  $y$ : it is only *after* they have been paired off that the relationship becomes unique—each  $x$  is paired off with whichever  $y$  he is and with none other.

So that if we say of two finite classes that they have the same number of terms if and only if there is a one-to-one correspondence between the two classes, we seem to have to distort slightly the meaning of one-to-one correspondence from the definition given by Russell. And it would be difficult to maintain that the sentences: "There is a one-to-one correspondence between A and B" and "A has as many terms as B" could always be used to replace each other without any change in meaning.

By using the first sentence we would normally be saying something about the *terms* of the class, and by using the second sentence we are only saying something about the *number* of terms.

To say that there are as many heads as there are tails on the pennies in this country is to say less than that there is a one-to-one correspondence between them: there might be many pennies with two heads and many with two tails and the equality might be fortuitous. And to say that there is one-to-one correspondence between the Fifth Form at St. Dominic's and the Sixth Form at St. Trinian's is clearly to imply more than that there is the same number in each.

Let us consider now how the language of correspondence is applied to collections in which the number of terms is indefinite—for example, the collection which contains all whole numbers and the collection which contains all even numbers.

We have seen that there is in the proper sense a one-to-one correspondence between these two classes. There is also, as we have seen, a two-to-one correspondence and a four-to-one correspondence, and it is clear that we could easily show that there is an  $x$ -to- $y$  correspondence where  $x$  and  $y$  are any positive whole numbers. This is simply to say that we can arrange the terms like this :

1	2	3	4	etc.
2	4	6	8	etc.

(a one-to-one correspondence)

or like this :

1, 2	3, 4	5, 6	7, 8	etc.
2	4	6	8	etc.

(a two-to-one correspondence)

or like this :

1, 2, 3, 4	5, 6, 7, 8	9, 10, 11, 12	13, 14, 15, 16	etc.
2	4	6	8	etc.

( a four-to-one correspondence)

or like this :

1, 2, 3	4, 5, 6	7, 8, 9	10, 11, 12	etc.
2, 4, 6, 8, 10	12, 14, 16, 18, 20	22, 24, 26, 28, 30	32, 34, 36, 38, 40	etc.

(a three-to-five correspondence)

Clearly, to get an  $x$ -to- $y$  correspondence we have simply to take the first  $x$  terms of the first class and the first  $y$  terms of the second class and so on.

It is perfectly intelligible and legitimate to express the fact that the terms can be grouped in these different ways by saying that between these two classes there is a one-to-one correspondence *and* a two-to-one correspondence, *and* a four-to-one correspondence, or simply by saying that there is an  $x$ -to- $y$  correspondence. Once we have understood that there is an indefinite number of terms in each

class, and have seen the ways in which they can be grouped, there is nothing in the least paradoxical or contradictory about this.

It is only when we start to say that the first class has as many terms, *and* twice as many terms, *and* four times as many terms as the second that we appear to get landed in paradox and contradiction. (And it seems so odd that anyone should want to say there are twice as many or four times as many when we have agreed beforehand that the number of terms is indefinite.)

Since it is a feature of two classes with an indefinite number of terms that any sort of correspondence is possible between them it is useful to make this into a definition of such classes. This is done by saying that a class has an indefinite number of terms (or is an infinite class) if there is a one-to-one correspondence between the class and some proper part of it.

This is perfectly intelligible and legitimate. But to go on to say that an infinite class is one which has the same number of terms as some proper part of it is a very different matter—unless it is merely meant that they have the same number of terms because the same word ‘indefinite’ can be applied to the number of terms of each.

It might still be argued that for indefinite numbers ‘the same as’, ‘twice as many as’, etc., are to be interpreted differently: they are to *mean* ‘having one-to-one correspondence with’, ‘having two-to-one correspondence with’, etc.

But if so, apart from the objections already urged, why use them when they merely introduce a confusing appearance of contradiction which need not be present when the language of correspondence is used?

And it is hard to suppose that it was only this that Russell was doing. Consider another remark which he makes about the number of numbers:

This new number is unchanged by adding 1 or subtracting 1 or doubling or halving or any of a number of other operations which we think of as necessarily making a number larger or smaller (*Introduction to Mathematical Philosophy*, p. 79).

Russell does not seem here to be explaining new ways of using the words ‘adding’, ‘subtracting’, ‘doubling’, etc.; he seems rather to be describing the behaviour of a rare and wonderful entity which is unaltered by the operation of doubling. But what does it mean to double the number of numbers? The number of numbers is not definite and there is nothing there to be doubled. Would it not be more reasonable (though less paradoxical), to say that the operations of adding, doubling, etc., cannot be performed when there is no definite number to perform them on?

All this is not merely a matter of deciding what form of words it is most convenient to use to describe a situation which we perfectly well understand. The use of inappropriate forms of words leads to misleading and muddled thinking.

I suggest that if we refrain from applying "the same as" and allied concepts to a number of terms which we know to be indefinite, and if we recognise that it is not possible to perform on an indefinite number such operations as doubling, not only are many apparent contradictions removed but also we will find it much easier to think clearly about what is really quite a simple matter. We will see that an indefinite number is not a positive 'thing' that is there, but a negative absence of definiteness.

In particular, remarks such as the following which appear in the same essay of Russell's on *Mathematics and the Metaphysicians* are seen to be nonsensical.

There are probably more points in space and more moments in time than there are finite numbers.

There are exactly as many fractions as whole numbers, although there are an infinite number of fractions between any two whole numbers.

There are probably exactly as many points in space as there are irrational numbers.

Nothing is gained here except the appearance of paradox, and it is gained at the expense of clarity of thought.

Various things that we may want to say about a series containing an indefinite number of terms, or an infinite series, can be said legitimately and intelligibly in the language of correspondence, and in this language we say things about the *terms*. But the language of simple arithmetical relationships is not legitimate, and it is both confusing and unnecessary. By using it we appear to be stating propositions about the *number* of terms and these propositions are inapplicable and nonsensical.

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## LAW OF CONTRADICTION AND EMPIRICAL REALITY

It may be said to be a fairly accepted presupposition of both science and philosophy that contradictory qualities cannot hold true of the same existent in any meaningful sense of the term. Philosophers have sometimes gone even to the extent of asserting the unreality of the empirical world because of its supposed possession of contradictory qualities. But, by and large, the research for empirical or rational coherence has always assumed the foundational significance of the Law of Contradiction for any understanding, on our part, of the world of empirical reality. It shall be our task in this paper to show some of the puzzling implications of this assumption and to suggest some doubts about its so-called self-obvious character.

The law of Contradiction is generally formulated as "A cannot be both B and not-B" where A and B are unrestricted variables. There are no particular values of A or B for which alone the statement is supposed to hold true. In fact, if any such restriction were to be imposed upon the formal expression of the law, it would immediately lose its inevitable, self-obvious, necessary character and become merely an empirical statement true only in certain cases of its exemplification.

The empirical interpretation of the formal law, however, immediately meets with a difficulty. Any empirical determination of the variables A and B raises immediately the question concerning the interpretation of the variable not-B. If, supposing, we empirically interpret it to mean "the table cannot be both red and not-red", then the question arises as to what exactly is meant by the term "not-red". Formally, the term "not-B" is supposed to include all that is not B. In fact, it is generated through what is known as 'division by dichotomy' and which, just because of this formal all-inclusive character, can never be invalid. But if we interpret the term "not-red" in this sense we will have to say that "the table cannot be both red and square", for "being square" is included under the term "not-red". Such a consequence, however, would hardly be acceptable to any person, for it is so obviously false that people would rather give up the law of contradiction than accept such a consequence.

The only way out of the difficulty is to restrict the interpretation of the term "not-B" to the determinates of the same determinable to which the term "B" may be said to belong. If, for example, the term "B" is interpreted to mean "red", then "not-B" includes only other determinates of the determinable of colour and not everything which is formally not B. To put it in other words, the counter-domain of B is empirical and not logical.

Such a solution, though generally accepted, is not without difficulties of its own. The term "not-B" interpreted, say, as green gives us merely the contrary and not the contradictory of B as was

supposed to be done in the formal presentation of the law. The self-obvious necessity of the law derived, however, from the contradictory relationship of B and not-B which is absent when the interpreted variables are related as mere contraries. It is, of course, possible to interpret the formal presentation in such a way as to be compatible with both a contrary and a contradictory interpretation. The law merely states that "A cannot be both B and not-B". Now, it is true of the contraries that they cannot both be true, though they may both be false. The term B and not-B can be interpreted as contradictories only when we take the Law of Excluded Middle also into account. It is only in conjunction with the statement "A must either be B or not-B" that the relation between B and not-B can be treated as one of contradiction. But on such an interpretation, the term "not-B" would not have an unrestricted domain limited only by the domain of the term "B". It will have to be restricted in some further sense, but whether such a sense can be found in pure formal logic, is difficult to say. In fact, on the formal plane, the Law of Contradiction cannot be torn apart from the Law of Excluded Middle and the terms B and not-B can never be treated as contraries but only as contradictories. It is only when we attempt an empirical or transcendental interpretation that the possibility of treating them as contraries and thus of giving up the law of excluded middle can arise. Brouwer, for example, has been able to deny the validity of the law of excluded middle in Mathematics only by treating it as concerned with some sort of entities which may be considered as transcendental in character. In contrast, Hilbert, who treats mathematics as a branch of or as identical with pure logic, cannot but treat B and not-B as contradictories implying, thus, the inevitable conjunction of the law of contradiction with the law of excluded middle.

The question of the inevitable conjunction of the law of excluded middle with the law of contradiction may, however, be raised in another way, as suggested by Mr. Om Prakash. Instead of asking whether the law of contradiction necessarily involves the law of excluded middle, we might ask whether, on the formal plane, the law of excluded middle necessarily involves the law of contradiction. In other words, does "A must either be B or not-B" involve that "A cannot be both B and not-B"? *Prima facie*, it seems that it must do so. But if we look closely, we shall find that the law of excluded middle is, in fact, a disjunctive proposition from which, according to the rules of disjunctive inference, the conclusion "A cannot be both B and not-B" cannot follow. It should be recalled that according to the rules of disjunctive inference by affirming either of the alternatives, we cannot deny the other alternative, though by denying one of the alternatives, we can always affirm the other. If, for example, we say in the major premise "A is either B or C" and in the minor premise "A is B", we cannot say in the conclusion "A is not C". The disjunctive proposition is *formally* supposed

merely to assert that A is characterised, *at least*, by one of the alternatives but does in no case *exclude* the possibility of its being characterised by *both*. The disjunctive proposition "A must either be B or not-B", therefore, distinctly allows for the possibility that "A *can* be both B and not-B". Unless, therefore, we are prepared to modify the rules of disjunctive inference, we must accept this consequence even if it appears as a manifest absurdity.

It may be suggested that the disjunctive proposition is generally an assertory proposition while the law of excluded middle is formulated as a necessary proposition. It is the force of the "must" which suggests that A cannot be both B and not-B. But in logic, it is the *form* of the proposition that determines the permissible consequences which can be drawn and, as far as I am aware, no such limitation on the disjunctive form of "Either—or" has yet been made.

Pratap Mehra has suggested another reason why on the assertion of the alternative "A is B" in the minor premise we cannot deny "A is not-B" in the conclusion. The conclusion, it is agreed, must not contain any proposition which has already occurred as a premise. Now if the major premise is "Either A must be B or not-B" and the minor premise "A is B", then the conclusion cannot be "A is not not-B" for it is just equivalent to "A is B". The same will be true if we assert "A is not-B" in the minor premise. "A is not B" in the conclusion would be equivalent to "A is not-B" in the minor premise.

This reason, however, is applicable only because of the nature of the alternatives B and not-B. The rule of the disjunctive syllogism is, on the other hand, more general and disallows the negation of the other alternative on the assertion of either on grounds of the implicational possibilities ascribed to the pure form designated by "Either . . . Or".

Thus, while the law of contradiction seems necessarily to involve, on the formal plane, the law of excluded middle, the latter cannot involve the former unless we be prepared to modify the rules of disjunctive inference in general. On any empirical or transcendental interpretation, on the other hand, neither need involve the other.

The difficulty arising from the behaviour of "not-B" as a contrary rather than a contradictory may, however, be met in another way.

It may be suggested that while the term "not-B" certainly does not include all that is formally not B, still it functions as the contradictory and not the contrary of B. This function it may be said to perform by virtue of the fact that it includes under itself not some particular determinate or set of determinates but rather *all* the determinates of the relevant determinable excluding, of course, the one denoted by the term B. If, for example, the term B is interpreted to mean "red", then the term not-B includes *all* the determinates of the determinable of colour excluding only the 'red'



denoted by the term B. The collective exhaustion of all the determinates between B and not-B would, then, be the ground of their being treated as *actual* contradictories even if they may not be formally so. But such a treatment can only be valid if it is assumed that A must necessarily be coloured for, if it is possible for A not to be coloured then it may neither be B nor not-B. This will only be possible if we treat white and dark as colours, for then it would be possible to say that A, if it is a physical object, must necessarily be coloured. However, even then, the necessity will only be empirical and not logical, for it is certainly not contradictory to say that "A is not coloured". Unless the statement "A is coloured" is taken to be an analytic statement, the B and not-B determinates of the determinable of colour cannot be taken as logical contradictories.

It should further be noted that it is on no *formal* ground that we say that an empirical object can be characterised by determinates of different determinables. If a table can be both square and red, the reason for this is not something formal but only experiential. Similarly, we suggest that if a table cannot be both red and green it is not because of any formal ground but only because experience shows it to be so. The formal law of contradiction, as we have just seen, can neither warrant the former nor support the latter.

However, even if we grant that the terms B and not-B should be confined to the determinates of the same determinable and that each physical existent must be characterised by some determinate of every determinable, it does not follow that the law of contradiction can safely be applied to empirical reality without any further trouble. The formal scheme "A cannot be both B and not-B" interpreted, for example, as "The table cannot be both red and green" can easily be shown to be wrong by pointing to a table whose one-half is painted red and the other-half green. It is bound to be objected that the table must be red and green *at the same point* and not in different regions of its surface. But the objection, though usual and familiar, is seriously mistaken. What was maintained in the interpreted proposition was that "The *table* cannot be both red and green" and not that the same point of the surface of the table cannot be both red and green. Obviously a point on the surface of the table is not the table and the statement that we made was about the latter and not the former. There seem, then, only two alternatives: Either we deny the necessary truth of the empirical interpretation of the law of contradiction with respect to physical objects or restrict the interpretation of the variable A to 'same points on the physical surfaces of objects'.

As the former alternative can hardly be chosen without involving a radical reorientation in our basic presuppositions of Science and Philosophy, the latter alternative is bound to be chosen. But even the drastic restriction on the interpretation of the variable A can hardly get us out of difficulties. The statement that "the same points on the physical surfaces of objects cannot be both red and

green" can easily be shown to be false by pointing to a uniform stretch of surface of some object which is at one time green and at the other time red. The interpretation of the variable A must, thus, be further restricted to "the same points at the same moments of time" which, then, according to the law of contradiction, cannot be said to be characterised by the different determinates of the same determinable.

This is hardly new. It has always been admitted that a thing cannot possess contradictory qualities at the same space and at the same time of its existence. But it has generally been overlooked that such an admission raises formidable difficulties of its own and makes the *a priori* applicability of the law of contradiction far from self-evident, as is generally assumed in most circles.

The first question that arises concerns the empirical meaningfulness of "the same point-instant" restriction on the interpretation of the variable A. If the term "point-instant" is something empirical then, obviously, it cannot be the theoretical limit of a possible infinite division but something as immediately observable as the ordinary objects or things of the world. But in such a case if it is pointed out that such a "point-instant" is *actually* characterised by different determinates of the same determinable then it cannot be argued that the very possession of these different determinates proves that it could not either be the *same* point or the *same* instant which was so characterised. If someone were still to argue in the above manner, it would only show that the person was not using the words in any empirical sense at all. In fact, he could hardly be refuted, for each time someone showed him such a case he could get away by saying that it could not have been the same point or the same instant of time. But he would have achieved this seeming invincibility by surrendering the basic fact about the inevitable empirical applicability of the law of contradiction, *viz.* the empirical interpretation of the variable A. If, for the truth of the law of contradiction, A must be interpreted in a non-empirical manner then the whole attempt to show the inevitable empirical application is given up at the very foundations.

The points, then, must be physical and not mathematical if we are to talk significantly about empirical objects at all. But if the points are physical, there seems no self-evidence about the supposed necessity of every such point being characterised by only one determinate of a particular determinable. Further, it will be extremely difficult to determine what exactly is meant by the term "physical point". If it is supposed to refer to the fundamental particles believed to be indivisible by the science of physics, then it would vary with the relevant developments in that science. If, however, field-phenomena are considered to be more fundamental, then the concept of "physical points" can only be considered in Whitehead's phrase as a "fallacy of simple location". Perhaps, Planck's basic quantum of action may provide that indivisible invariant which the

notion of physical points requires. But whether it would suitably fulfil the function for which it is required, is more than we can say.

Even if a satisfactory correlate of the notion of physical points can be found in the science of physics, it would, we think, not serve the purpose. The law of contradiction would, then, apply *only* to sub-atomic particles and not to atoms, molecules or other microscopic or macroscopic objects. But, in that case, the law will not be applicable to most objects and, though certainly empirical at one level of its application, it would mostly be irrelevant for man's concern with the macroscopic world of his sense-experience. In fact, for the purposes of the law of contradiction, the object is atomistic in character—a view that does grave injustice to the unity of the object. A table, for example, is a unitary object and not merely a sum total of the so-called physical points in the universe.

In whatever manner we may solve the difficulties resulting from the notion of the "same point", those resulting from the co-ordinate concept of the "same instant" will still remain. The question about the logical versus the physical interpretation and the difficulties concerning the latter will be the same as in the former case. There will, on the other hand, be the added difficulty of defining 'simultaneity' for different observers.<sup>1</sup>

Further, the concept of the "same instant", whether interpreted logically or physically, would make the application of the law of contradiction to empirical reality either impossible or irrelevant. If it is *only* at the *same instant of time* that A cannot be both B and not-B, then if A *endures in time*, the law cannot apply to it and if it persists only for an instant, it is irrelevant to it. If an empirical object is considered as a patterned unity enduring through multitudinous instants of time, then it is impossible that the law apply to it. On the other hand, if the thing itself is considered as instantaneous or momentary, the law, though true, is irrelevant—for, the thing at every different moment is a different thing.

It has not been noticed by thinkers that the overriding phrase "at the same point of space and at the same instant of time" involves a Humian or Buddhistic view of empirical reality which cuts at the very root of the significant applicability of the law of contradiction to that realm. For, if a thing is no thing but merely a point-instant series of events then it surely can be characterised by B and not-B at different instants of time. If it is suggested that the thing at a different instant of time is a different thing, the situation would be no better. In that case, it will be a tautology to say that the law of contradiction necessarily applies to empirical reality.

<sup>1</sup> I am not quite sure whether the same corresponding difficulty will not arise with respect to the operational meaning of the concept "same point" for different observers. It is, of course, possible that a corresponding difficulty may arise with respect to the operational meaning of the concept "same point" for different observers. But I am not quite sure about it.

The statement, while true, will be of no significance, for everything would last only for an instant and then, for purposes of contradiction, would be no more.

The point-instant limitation on the interpretation of the variable A leads to another serious difficulty if it is interpreted in the rigorous mathematical manner as it should be to make the applicability both self-evident and necessary. It is well-known, following Cantor's discovery of the transfinite numbers, that the number of points on a surface, however large or small, is the same. Now if a thing must be treated, for the purposes of the law of contradiction, as consisting of nothing but points, it follows that all things are empirically the same. This, however, is difficult to accept, for it is equally well-accepted that any argument which leads to infinite consequences concerning physical objects is invalid. In Professor Ryle's terminology, we are harnessing the wrong horse to the wrong carriage. But this is just our contention that the empirical applicability of the law of contradiction requires us to perform this impossible feat. Infinite analysis in the field of physical objects is bound to lead to paradoxical results, but this is just what is demanded if the law of contradiction is to apply to empirical reality. The dilemma can be avoided only by giving up the demand, but that would involve, in its own turn, new difficulties.

The empirical interpretation in terms of "the same point of space" leads to another difficulty with respect to those empirical objects which cannot, possibly, be spatially characterised. Mental phenomena, it is more or less agreed, cannot be characterised with respect to their spatial co-ordinates. Even if they are considered to possess some extensivity or spread-outness, it would be difficult to regard them as located in space in any ordinary sense of the term. If, then, the point-instant interpretation of the variable A is held necessary, then the law of contradiction cannot apply to, at least, one realm of empirical reality, viz. the mental phenomena. A possible alternative will be to limit the necessity to the "instant" interpretation only. But, then, at the same instant B and not-B could characterise the same thing at different points and thus invalidate the law of contradiction. Either, then, we have to deny the applicability of the law of contradiction to mental phenomena which are undoubtedly empirical or we must maintain that they can be as much characterised by spatial determinations as any other physical object.

The common assumption that the law of contradiction must necessarily apply to empirical reality seems, thus, to be so full of puzzling features that it appears surprising how it could have been accepted as self-evident by so many thinkers in the field of philosophy. Far from being self-evident, it reveals, as we have seen, problems both of a formal and an empirical nature. At the formal level, the problems centre around the interpretation of the variable not-B and the relations between B and not-B. At the empirical level, the issues

centre round the interpretation of the variable A. The "point-instant" interpretation without which the empirical applicability of the law cannot be defended makes, in its own turn, the application either impossible or irrelevant. The cumulative effect of all these difficulties is, we suggest, to make impossible the application of this law. Either there is something radically wrong with the arguments we have given or this age-old self-evident axiom of science and philosophy is only a hollow myth. There is no other alternative, and unless we are shown that the former alternative is the correct one, we cannot but choose to assert the latter.

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## THE PROVINCE OF LOGIC

MR. E. J. LEMMON has drawn my attention to a mistake on page 253 of the paper called "The Province of Logic" which I contributed to the volume *Contemporary British Philosophy* (Third Series). The passage (beginning on l. 12) in which I compare my rules for logical development with Gentzen's version of intuitionist logic should be made to read as follows: "The difference is not due, however, to the addition of any special axiom, nor yet to any new way of eliminating the negation sign, but to the alteration of rules (5) and (7) in a way that seems almost inevitable when once the notion of inference has been replaced by the wider notion of development. If anyone wishes for an intuitionist theory of development, he can have it by dropping our rules (5) and (7a) in favour of Gentzen's (5) and (7). But in this context Gentzen's (5) and (7) are obviously anomalous, since they do not introduce '⌈' and '⌋' in a straightforward fashion, corresponding to the way in which those signs are eliminated, but employ the device of suppositions, which is no longer required for any other rule."

There is also a mistake on page 256 where I say that the rules given on the previous page are sufficient for the elimination of redundancies from statements of involution. The two sentences after the central figure on page 256 should therefore be deleted, and on page 255 after the printed schema (ii), now renumbered as (iia), there should be added a new schema (iiβ),

$$\frac{\Gamma, \Delta, \Delta/\Theta, A, A}{\Gamma, \Delta/\Theta, A}.$$

On page 256 the passage after "Chrysippus" in l. 8 should then be made to read: "Thus the two parts of the second rule make clear that in summarizing a development we may rearrange formulae freely and eliminate redundancies on either side of the solidus. Involution is a relation between *sets of propositions* and, as Leibniz said of one of his calculi, we take no account here of order or repetition. Similarly the third rule allows for additions. . . ."

On page 248 a "Q" has been omitted after a negation sign near the end of the proof of rule (14). On page 356 "Γ" should be interchanged with "Θ" and "Δ" with "A" in the central figure for the sake of uniformity with the lettering of rule (iv) on the previous page, and in the seventh line below the figure "now" should be replaced by "more". On page 257 "(ii)" should be added after "(iv)" at the widest part of the proof, and the bracket should be placed after instead of before "R" at the end of the next to the last line of the proof.

The responsibility for all these mistakes (and for any others that may be found in the paper) is, of course, mine.

WILLIAM KNEALE

## VII.—CRITICAL NOTICE

*The Illusion of the Epoch.* By H. B. ACTON. Cohen & West Ltd.  
Pp. 278. 18s.

MARX wrote at a time when metaphysical foundations were fashionable, and a large part of his aim was to provide metaphysical backing for contemporary socialist criticisms of the political and economic organisation of Western Europe. He would certainly have rejected the term 'metaphysical' and have claimed that his foundations were not at all metaphysical but were scientific; but equally certainly he would have rejected any claim that they might be destroyed by experimental evidence. It is therefore not difficult to show that his doctrines have the same emptiness as those of the idealists and the undialectical materialists against whom he conducted such a vigorous verbal war. The 'war' was one in which there could be no victors, no vanquished, and no casualties.

Hence one may wonder whether a detailed dissection of the moves made by Marx and his disciples Engels and Lenin is of any importance today. Professor Acton thinks that it is, and he is right. We can none of us be quite sure how far the present rulers of Russia and China regard their Marxist paraphernalia merely as window-dressing, but it is obvious that many of their followers take them seriously. They believe that Marx and Lenin discovered and stated important truths about the world which bourgeois critics persistently ignore or misrepresent; and it is therefore high time that a philosopher of standing should go through the often wearisome labour of analysis and criticism. Granted this, a difficulty at once presents itself. How far is the job to be done one of straightforward philosophical analysis and how far is it one of the history of communist thought on what were once regarded as real philosophical puzzles? Acton seems to me not to have come to terms with this, and perhaps it is impossible to do so without writing at inordinate length. Evidently, however, the existence of the umbrella word 'Marx-Leninism' and the assumption of the faithful that the works of Marx and Lenin constitute a single coherent doctrine is a weak justification for considering *Capital*, *Empirio-criticism* and *Anti-Dühring* as belonging to a system at all. It is *prima facie* most improbable that Marx and Lenin would have failed to quarrel violently with one another about almost everything if they had had an opportunity. They were like that.

It would be a mistake to make too much of this awkwardness. There is a sense in which one can usefully talk of a *Weltanschauung* shared by Plato and Aristotle but rejected by medieval philosophers, and in this sense 'Marx-Leninism' does stand for a general point of view shared by Communists and rejected by Capitalists at the present



day. But one should beware of demanding agreement in detail on either side of the curtain.

Acton divides his criticism of Marxism as follows (and I shall follow him in using this abbreviation, though with the reservation made above) :

I. *Dialectical Materialism*

- (i) Marxist Realism.
- (ii) Marxist Naturalism.
- (iii) Marxist Dialectics.

II. *Scientific Socialism*

- (i) Historical Materialism.
- (ii) Marxist Ethics.

I (i) *Marxist Realism*

Marx and Engels had very little to say about epistemology, and at first sight this is not surprising. It seems a long way from *Esse est percipi* to the barricades. Marxists, however, do not agree. They think it important to maintain that all religious views, and especially the doctrines of Christianity, are ideological fictions propagated by the intelligent rich in order to reconcile the uneducated poor with the existing economic system. In addition it seems clear to Marxists, who are here the heirs of eighteenth century materialism, that scientists as a class are anti-Christian, and that therefore they should be reliable allies in the battle against Christianity.

In the nineteenth century this was a reasonable assumption, but by 1909 the dichotomy between religion (Fideism, as Lenin called it) and materialism was breaking down. It was challenged by the type of phenomenalism associated with Mach and Avenarius, and known as empirio-criticism. Now the trouble about this view (and also about its developments after Lenin's death) was that the holders of it claimed to be the philosophers of science and yet rejected materialism. This was a stab in the back, and Lenin, though admittedly a complete amateur at philosophy, felt that he must take strong action. Hence he wrote *Materialism and Empirio-Criticism*. Its purpose was to demonstrate that phenomenalism was in fact the view of Berkeley in a sophisticated disguise. Berkeley was a fideist, therefore phenomenalism was inconsistent with orthodox Marxism.

In defence of this thesis, he found himself committed to refuting phenomenalism as a philosophical view, and it is on this level that Acton expounds and criticises him. Not surprisingly, Lenin comes out of this encounter pretty badly. He did not know the moves, and Acton, with the discussions of phenomenalism which studied philosophical literature in the nineteen-thirties at his disposal, gives him a very rough time. I cannot help thinking, however, that the attack does less than justice to what Lenin meant, though, as a peppery beginner, he put it very badly.

Lenin was right, and Acton agrees that he was right, in denying that the reduction of statements about material things to statements about sense-data is practicable. Acton, however, considers that Lenin was wrong in describing our experience (including our thinking) as a picture (*Bild*) of reality. In a rather obvious way, so he was. But *Bild* is not equivalent to 'picture', and I am inclined to think that the doctrine of Wittgenstein's '*Tractatus*' (*Wir machen uns Bilder der Tatsachen*) was nearer to what Lenin was trying to say than is the very crude copying theory which is attributed to him here. He need not even have rejected the conclusion that this kind of epistemological talk is nonsense, but that it is quite an important kind of nonsense all the same.

### I (ii) *Marxist Naturalism*

Marxist naturalism, as Acton calls it, might have been more suitably described as Marxist positivism, and, since it is both historically and logically prior to Lenin's excursions into epistemology, it might well have been treated first. The central point of it is the revolt of Marx against the speculative idealism of Hegel, but exposition and criticism are difficult because Marx was always more interested in smelling out heresy than in giving a systematic formulation of his own view. This is not surprising because his own view was so generally accepted in progressive circles in the mid-nineteenth century. Comte, J. S. Mill, George Eliot and the Young Hegelians all took it for granted that Christianity was finished, so Marx had no need to labour this point. What mattered to him was that the executioners, especially Feuerbach, should not do their job clumsily and thereby obscure the social implications of it. The theme was that science had killed speculative philosophy and with it religion (especially Christianity), but science must itself be watchful. Unless it took over the good, dialectical ingredient in Hegel, it would become involved again in the idealist conclusions which Berkeley had imposed on Locke and the French materialists. This is the general position from which Lenin's attack on empirio-criticism is derived.

What is wrong with speculative philosophy is that, by giving thought the primacy over sense experience, it ignores the necessity for empirical verification as a test of truth. In Marxist language, it ignores the unity of theory and practice. This is not at all thoroughly worked out, and, as Acton shows, 'practice' is used by Marxists in several different ways. I think he makes rather heavy weather over this. Marx had something to say which was true and important, though he certainly did not put it as well as Kant had done some time before. But Marx did not know about Kant except as misrepresented by Hegel.

Anyway, it seemed clear to Marx and Engels that by demolishing the claim of speculative philosophy to achieve knowledge of matter

of fact, they had *a fortiori* disposed of the claim of religion to achieve truth at all. Here they were too simple minded, but I do not think that Acton's criticism of them is effective on this point. Certainly it will not do to treat 'God exists' in the same way as 'Abominable snowmen exist'. But it is no solution of the puzzle to suggest, as Acton does, that God might have to be postulated as an unobservable entity (like positrons) to assist scientific discovery or even to unify scientific hypotheses.

### I (iii) *Marxist Dialectics*

Finally, there is the depressing talk about quantity changing into quality and the negation of negation. Acton makes short work of this. He might, perhaps, have given Marx and Engels the credit for seeing obscurely, as Hegel himself had done, that the logic of Aristotle was not an adequate tool for nineteenth century thought. Something had to be done about relations and tenses, though the dialectic was not at all the right answer.

### II (i) *Historical Materialism*

The idea that human behaviour in general and human history in particular might be scientifically treated was not a new one and Marx did not claim to have originated it. Like Marxist positivism it was part of the climate of opinion in progressive circles at the time when he and Engels developed their views. Nor was his belief that moral and political principles reflected the economic facts of life in particular societies a novel one. Such ideas were indeed in conflict with theological conceptions of moral and political obligation and with the Hegelian doctrine of the State, but they were widely entertained. The treatment of human history as the story of successive cultures or ways of life was indeed under Hegelian patronage.

What Marx attempted to do was to replace the Hegelian idea of history as the progressive unfolding of mind by a materialist idea of history as a series of jumps based on technological innovations, and to reduce what Hegel had considered fundamental, namely the development of moral and political ideas, to the level of an ideological superstructure. In fact he here followed out his general plan of considering mental events as the product and reflection of a material substructure.

It is perhaps unnecessary to dwell on the unprofitable results of this type of reduction. Acton's analysis of the confusions which flow inevitably from the language of 'ideology', 'material conditions of production' and the rest of it is clear and thorough. It requires no comment, especially as historicism is in disrepute nowadays and criticisms of Marx's somewhat confused expositions of it are therefore superfluous except for orthodox Marxists.

II (ii) *Marxist Ethics*

Although Marx and Engels provide no systematic discussion of ethics, they maintain throughout two propositions which are at first sight inconsistent with one another. The first is that ethical doctrines are part of the ideological superstructure of society, that is, they are part of the apparatus by which the underlying class struggle is concealed by the rulers both from the exploited and from the politically uneducated section of the exploiters. Hence it seems to follow that, for those who understand scientific socialism, moral appraisals are without significance. But, on the other hand, words like 'exploitation', which are heavily charged with moral disapproval, are essential if the Marxist attack on capitalism is to have any force. But this is not such a hopeless muddle as it seems. Acton recognises that the conception of the classless society in which 'the government of men is replaced by the administration of things' is important in this connexion but finds great difficulty in seeing what this could mean. I think the answer, though it is still far from clear, might be this: Marx, and plenty of social reformers, saw clearly that a great deal of anti-social behaviour was the direct product of capitalist economic and political organisation. Theft and prostitution were the obvious targets of this attack. To say that these were wrong was (and is) pointless. What was (and is) required was the removal of their causes. But, if Marx was right, the causes would be removed only by the advent of the classless society. At that stage a very large number of moral appraisals in current use would cease to have any reference. There would, however, still be rules of social behaviour with penalties for infringement. To say that this is to make a distinction without a difference is to go much too far. There is a big difference between the job of a referee and that of a policeman; and if the remark about the replacement of government by administration is interpreted thus, it may well be called over-optimistic, but I do not think it is unintelligible.

*Conclusion*

Acton ends with a dialogue in which he sums up his view. For some reason, nobody since Hume has produced a philosophical dialogue which I find anything but irritating, and this one is no exception. The point of it, however, is clear enough. It is that 'Marxism is a mixture of two philosophies which cannot consistently go together, positivism on the one hand and Hegelianism on the other'. Hence the concluding remark that 'Marxism is a philosophical farrago'.

In a way this is perfectly true, and Acton's detailed analysis has revealed the flaws. But all the same the conclusion is inadequate. The fact is that any important thinker can be taken to pieces in this way. Kant, for instance, substituting 'Locke' for 'positivism' and 'Leibniz' for 'Hegelianism', might with equal plausibility be

said to have produced a farrago. He is often accused of having done just that. What Marx really achieved, however, was a revolution in the outlook of sociologists. His categories, 'class-war', 'surplus value', etc., were inadequate; his machinery of 'quantity', 'negation of negation'; and his antithesis of 'idealism' and 'materialism' all deserve technically the unkind things which Acton says about them. But he was a genius for all that. He did for sociology very much what Freud, another authentic nineteenth-century genius, did for psychology, and it should not be forgotten that his work was the inspiration of every Social Democratic movement in Europe as well as of the Bolshevik Party in the U.S.S.R. Part of the trouble inherent in writing about 'Marxism' instead of about 'Marx' is that the writer tends to overemphasise the ossification of his thought which less competent successors have brought about.

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## VIII.—NEW BOOKS

*The Theory of Proper Names. A Controversial Essay.* By SIR ALAN H. GARDINER. 2nd edn. (1st edn. 1940). Oxford University Press, 1954. Pp. viii + 76. 8s. 6d.

THIS essay has two purposes: to defend a modified version of J. S. Mill's account of ordinary proper names; and to attack Russell's theory of logically proper names.

Mill said that proper names lack connotation. It would be more accurate to say that the functioning of a proper name is neither helped nor hindered by any connotation which it may happen to possess. But Sir Alan Gardiner introduces what I take to be this modification in a strange way. "A proper name is a word or groups of words recognized as indicating or tending to indicate the object or objects to which it refers by virtue of its distinctive sound alone, without regard to any meaning possessed by that sound from the start, or acquired by it through association with the said object or objects" (p. 43). But what is "by virtue of its distinctive sound alone" doing in this definition? General names too rely on their distinctive sounds. However, "it makes a vast amount of difference whether the distinctive sound is a self-sufficient means of identification, or whether it has to be assisted, as in general names, by a consideration of the meaning" (p. 39). But this is Mill's difference (though, perhaps, he did not state it accurately enough). The point of "by virtue of its distinctive sound alone" is purely negative, and is completely explained in the clauses which follow. The phrase would certainly seem to have an additional positive content if the word "alone" were forgotten, and "by virtue of its sound" began to masquerade as an *alternative* to "by virtue of its meaning". But this view is not to be found here. However, he does try to give it an additional positive content in another way. For he claims that the sound of a proper name forces itself on our attention more than the sound of a general name (pp. 35-38). S. Ullmann, reviewing the first edition of this essay in *Archivum Linguisticum*, iv. i. (1952), observes that this claim is difficult to verify. It seems to me to be incorrect. When I hear the name *York* I think of the city as often as I think of the connotation of the word *cathedral* when I hear that word. Ullmann's criticism led Sir Alan Gardiner to alter his definition so that it no longer implies that people *recognize* that proper names function by their distinctive sound alone (see "Retrospect 1953" at the end of the essay). But this alteration is not enough. For it is not even a fact that they function by their distinctive sound alone, if this is taken to imply that consciousness of the sound of a proper name predominates more than consciousness of the sound of a general name. It does not predominate more. Or does the alteration remove this implication too? In any case, he did uphold the implication in the essay. Why? Perhaps because he believed that thinking of the city would be thinking of the connotation of *York*. Some evidence for this suggestion is to be found in his remarks on Jespersen's view (p. 31). But, however this may be, his attempt to give the phrase an additional positive content breaks down. Nor can I see any way in which this could be done successfully. Consequently the modification of Mill's account, in so far as it is correct, seems to me to amount to the point made in the second sentence of this paragraph.

Russell set out his logical innovation in words which sometimes suggest that it is a piece of philological description. Sir Alan Gardiner produces a series of pungent criticisms which establish that, as description, it is not only incorrect, but also a long way from the truth. He disclaims familiarity with Russell's philosophy, but he suggests that the cause of this aberration is Russell's theory of acquaintance (p. 65).

It is the merit of this essay that it collects a variety of material, enough to upset not only artificial theories like Russell's, but also many of the theories which really are meant to be empirical results of surveying this border-country between philology and logic. For instance, it is argued that some proper names are plural (e.g. *the Andes*), and that some singular names are not proper (e.g. *the sun*). And it is strongly emphasized that, since many factors combine to make a name absolutely proper, there are many directions of possible deviation.

The weakness of the essay is that its theoretical apparatus is not sufficiently fine and accurate. Even a fine distinction will, in the end, leave some of the material unsorted. But the distinctions offered here are sometimes too faulty to be used with any confidence. The most conspicuous example of this is the antithesis on which the modification of Mill's account depends. But there are others. The difference between a word and a name (not necessarily a proper name) is said to lie in the direction of thought, which is, in the first case, from the word to its meaning, and, in the second case, from the thing to its name (p. 7 and p. 56, n.); but a lot of discriminating would have to be done in order to discover the sense in which this is true. Also there is a good deal of obscurity on pages 9 and 10, much of which could have been avoided by the use of inverted commas and of the distinction between type and token, or by equivalent devices.

D. F. PEARS

*The Life of David Hume.* By E. C. MOSSNER. Nelson, 1954. Pp. 683, 42s.

*New Letters of David Hume.* Edited by R. KLIBANSKY and E. C. MOSSNER, The Clarendon Press, Oxford, 1954. Pp. 253. 30s.

*The Life of David Hume* is the third major biography of Hume, the two earlier being those by John Hill Burton (1846) and J. Y. T. Greig (1931). Like them it is associated with a new edition of Hume's letters. *New Letters of David Hume* contains over 100 hitherto unpublished letters. The editors do not claim that these letters reveal any startling new discoveries,—but many of them make interesting reading, and invite reconsideration of such questions as Hume's stature as a diplomatist, and his motives and justification for permitting the publication of the documents concerned in the quarrel with Rousseau.

Hume's official letters to H.M. Secretary of State, written when he was *Chargé d'affaires* in Paris in 1765, were consulted, but not published, by Greig, who concluded in his usual abrupt manner that Hume was made game of by the French. Mossner and Klibansky publish these letters, suggest that they require a less condescending view of Hume's abilities, and in their introduction give a brief but lucid account of the three principal questions with which Hume had to deal—the dismantling of the Dunkirk fortifications, the Canada Bills, and the Newfoundland Fisheries. The



first question was not settled till 1783, when England gave up all treaty rights to prevent the fortification of Dunkirk, the second was settled by direct negotiations in London, not through diplomatic channels, and the third was not finally settled till 1904. At least it can be said that Hume did not fail where other diplomats succeeded.

Perhaps the most interesting light shed on the Rousseau affair is provided by a letter to John Crawford, dated 20th December, 1766. Here Hume confesses that he is not pleased with what he has done, but points out that he had to choose between being blamed for cruelty to Rousseau in publishing the documents of the quarrel, and being blamed for being the "Calumniator and false perfidious friend" that Rousseau made him out to be. "There was no proportion between the one blame and the other", says Hume.

Mossner, in his *Life*, clearly recognises the strength of Hume's concern both for his moral integrity and his moral reputation. It was not easy, in the eighteenth century, both to be and to be thought just. Hume desired both these things, and strove for them manfully throughout his life, just as in philosophy he strove, less successfully, both to discover the truth, and to convince others of it.

Philosophical readers of *The New Letters* may care to look into the following passage from a letter to Henry Home (Lord Kames) dated 24th July, 1746.

"I likt exceedingly your Method of explaining personal identity as more satisfactory than anything that had ever occurred to me."

The reference is said to be to Kames' *Essays on the Principles of Morality and Natural Religion* (Edinburgh 1751) pages 231-236.

Professor Mossner's *Life* is a long, scholarly, leisurely but eminently readable work. Though on most questions he is content to cite the contemporary evidence and allow it to speak for itself, without indulging in the snap judgments which enliven Greig's book, his own view of Hume is plain and unmistakable; Hume was "the greatest mind of the Enlightenment", and, though not a saint, judged by human standards, as he himself would have wished to be judged, he was near blameless. The aim of the book is not primarily to assess Hume's importance as a philosopher or a historian, but to present a picture of him as a man, and a manly man. Hume's advice, "Be a philosopher, but with all your philosophy be still a man", is frequently quoted, though the reference is not given. This view of Hume is at first sight a little surprising; one thinks of his caution, his vacillations, his alleged complacency, his love of peace and ease. But there is a case for this view, and Professor Mossner makes it well in his pictures of the ten years of arduous study, battle with ill-health, fearless thought and striving for clear expression that gave birth to the "Treatise of Human Nature", and of Hume's life-long endeavour to live up to the moral ideals he formed from his early readings of the ancient philosophers.

The author only professes to give so much account of Hume's philosophy as is needed to understand his life. In doing so he is wise to rely largely on quotations from Hume's Abstract of the Treatise. He does, however, on page 76, offer us his own description of "the uniquely Humian contribution to philosophy". It was, he says, "The extension of sentiment or feeling beyond ethics and aesthetics (to which it was limited by Hutcheson) to include the entire realm of belief covering all matters of fact". True as far as it goes, this summary fails to bring out the importance

of the threefold distinction, once for all laid down by Hume, between statements of logic, statements of fact and judgments of value. Hume's own words, quoted on page 260, are a memorable epitome of the lesson he taught us. "There are many different kinds of certainty; and some of them as satisfactory to the mind, tho' perhaps not so regular, as the demonstrative kind."

"There would appear to be a logical error on page 606, where Dr. Johnson is accused of "illogic" for suggesting that "on his own principle of annihilation" Hume had no motive not to lie when he said that he had no fear of annihilation. Dr. Johnson would have been illogical if he had been suggesting that Hume really believed in a future life; but, as far as I can see, he was only suggesting that Hume really was afraid of dying, though he said he was not.

To return in conclusion to the merits of the book, I find the chief of these to be the complete and convincing picture it gives of the world in which Hume lived, in Scotland, England and France, and the sense the reader has of having been introduced to an intimate and respectful acquaintance with a great and lovable man.

D. G. C. MACNABB

*David Hume: Writings on Economics.* Edited and introduced by  
EUGENE ROTWEIN. Nelson, 1955. Pp. cxi + 224. 30s.

UNTIL late in the nineteenth century, almost all academically trained economists in this country were also philosophers. But although many of them claimed their political economy to be an application of their philosophy, it is doubtful whether there was a logical link between the two. (Such a link would also provide a return journey from economics to philosophy.) Hume intended his writings on economics to be essays in the general science of human nature, but his contributions are interesting because he neglects this intention. Professor Rotwein, in his long, careful and searching Introduction to this new edition of Hume's economic writings, takes the opposite view. He argues that, although in dealing with specific economic problems Hume adapted his analysis to them, the unifying principle in his analysis can be found only in his philosophy. We are asked to take Hume seriously when he declared at the outset that he intends to employ his principles of human nature as a basis for exploring economics. Having explored the structure of human behaviour by a method which seems to combine elements of the philosophy of natural law with a stress on historical change and on environment, Hume proceeds to apply his discoveries (Rotwein argues) to particular fields of experience. (The wide appeal of English and Scottish political economy may be partly due to this apparent blend of analytical and historical approach.)

Rotwein develops his argument by relating Hume's economic analysis to (1) his economic psychology (Hume's discussion of what we would now call incentives is said to be joined to the argument of the *Treatise*); (2) political economy proper (it is said to be related to Hume's "natural history"); and (3) his economic philosophy (the moral appraisal of a commercial society by comparison with earlier forms). Rotwein's argument is attractive, but not entirely convincing. The essays, with some interesting letters, are there for anybody to form his own judgment. (At least those who take their PPE seriously will have to do that.)

In the history of economic thought, Hume was an outstanding member or what now looks like the "anti-Keynesian Revolution" of the classical economists. He was amongst those who rejected the prevailing notions of the importance of money and turned to "real" explanations. His expositions of the Quantity Theory and of Balance of Trade adjustments contributed to the shift of government policy from regulating money and trade to men and production, although, as one would expect, he was much less dogmatic than many of his successors. Some of the features which strike us as particularly modern in his writings may be regarded as either pre-classical remnants or anticipations of post-classical thought. There is a clearly developed multiplier analysis, there are qualifications for hoarding in his theory of money, and for income and exchange rate adjustments in his foreign trade theory, and a discussion of the influence of the supply of money on interest rates. Kohelethians<sup>1</sup> will find hints of Ricardo's theory of rent, elements of Malthus' population theory, and anticipations of Herder, modern psychological anthropology, Toynbee's challenge and response, concern with under-developed countries and the problems of growth, and a full discussion, in plain language, of the demonstration effect, the diversification effect, and other fashionable jargon. Besides these morsels for anticipation-hunters and besides Hume's well-known solid contributions to economics, there are amusing observations, fascinating reflections (particularly in the essay "Of the Populousness of Ancient Nations") and witty polemics. Not only philosophers and economists have cause to be greatly indebted to Rotwein for this edition and for his very helpful Introduction.

PAUL STREETEN

*The Controversy about the existence of the world (Spór o istnienie świata).*

By ROMAN INGARDEN, vols. i. and ii, 1947-48. Polish Academy of Sciences and Letters.

In his *Histoire de la Philosophie Contemporaine en Europe* Bochenski describes *The Controversy about the existence of the world*, by Roman Ingarden as one of the most important publications in recent years. Being written in Polish this work is unfortunately only available to a small number of philosophers.

Ingarden first became generally known by his *Literarisches Kunstwerk* (Halle, 1932, reviewed in *MIND* vol. xli). He is also not unfamiliar to the readers of Husserl's *Jahrbuch für Philosophie und phänomenologische Forschung*. Even at that time, however, his original philosophical interest prevailed, displaying his independence of the Husserlian school. In a series of essays written between the two wars he opposed the spirit of mathematical logic which was then dominant in philosophy, especially in Poland. His work contained many original and widely separated insights which form the basis of his later inclusive philosophy.

In the treatise under review Ingarden establishes the foundation for the complex of problems arising in the discussion of realism *v.* idealism. His monumental endeavour begins with the observation that the recurrent

<sup>1</sup> "The words of Koheleth, the son of David, king in Jerusalem . . . there is no new thing under the sun. Is there anything whereof it may be said, See this is new? it hath been already of old time which was before us" (*Ecclesiastes*, i: 1, 9-10).

disagreement about the existence of the real world both between idealists and realists and within each of two camps indicates an inadequate formulation of the problem itself. To analyse this problem Ingarden is led to study all philosophical questions arising in this controversy throughout the evolution of philosophy.

The first major result of this investigation is a classification of philosophical questions according to the nature of their objects and the method which these objects require. Whether the real world and beings *actually* exists or not is what Ingarden calls a "metaphysical question". The more basic question, however, of what should be meant by "real world" is regarded by Ingarden as "ontological". Starting from the Cartesian postulate and having given a new form to Plato's realism, Ingarden seeks to analyse what beings are in terms of their ideal structures in the "contents of corresponding ideas". These structures are considered merely as possible, without any indication whether so constituted particular beings actually exist or not.

The first two volumes of the treatise, to be followed by others, are devoted to an ontological inquiry concerning the nature of the real world. In this connexion Ingarden analyses all types of beings which we know: *individual autonomous beings* (ideal, i.e. τα μαθηματικά of Plato, or real e.g. concrete physical objects, the world as a whole), *ideas* (i.e. Platonic ideas), *merely intentional beings* (e.g. a literary work of art), and *pure qualities* (e.g. the pure quality of redness).

Moreover, in the history of philosophy the idealists in disputing the independent existence of the real world have considered the real world as relative to "pure consciousness". The latter constitutes therefore the second pole of Ingarden's investigation. The first volume presents the ontological elaboration of the existential features of beings. Following Aristotelian tradition Ingarden analyses them in terms of their structural composition, as differentiating the *really*, *ideally*, *merely intentionally* or *absolutely* existing beings and distinguishing corresponding "modes of existence". The latter are respectively constructed out of the following "moments of existence": *autonomy-heteronomy*, *disconnection-connection*, *dependence-independence*, *originality-derivedness of existence*. The temporal character of existence treated *in extenso* gives rise to further moments of existence and approached from the ontological point of view contributes original insights to the problem of temporality so widely discussed since Einstein.

The recognition of the *a priori* laws of connexion and exclusion between the possible components of a structural organisation of beings permits Ingarden, after a review of all *a priori* possible formulations and solutions of the idealism-realism problem, to eliminate the self-contradictory ones as well as to give precision to the confusedly used concept of "dependence" and "independence" of the world in respect to the consciousness. Nevertheless, at the end of these existential analyses there remain still fifteen formulations and consequently the same number of possible solutions (naïve realism and idealism are excluded).

In the second volume the analysis of *forms* of beings complements the analysis of modes of existence so as to yield a complete understanding of the structures of beings. The antithesis is between *form* as abstractly distinguishable pattern of organisation and *matter* as "what is organised". Ingarden's "form" and "matter" concepts present an original and penetrating elaboration constituting an important contribution to the Platonic-Aristotelian tradition.

The centre of interest are the fundamental forms of the world and of pure consciousness. This, however, is prepared by an analysis of the forms of ideas, merely intentional beings, autonomous individual beings, events, processes and states of affairs, giving important contributions for the refutation of the idealistic assumptions. These successive analyses of forms should not lead to anything like Anaxagoras' world of "all things together". Ingarden introduces processes within the structure of beings explaining by this means their changeability and destructibility. In this respect much attention is devoted to Hume's problem of the persisting identity of a being during the passage of time.

It is impossible within the limits of this review to give even the most essential features of such elaborate notions as those of the world (approached in terms of a *region* of real beings) and pure consciousness (discussed in terms of the *act* and *stream* of consciousness). However, it should be pointed out that at the end of the second volume only two possible formulations of the idealism-realism problem remain: *absolute creationism*, according to which the world would be *autonomous* and *independent* but *derived* from pure consciousness, and *realistic creationism*, according to which the world would be as in the previous case, but *dependent* on pure consciousness.

The final problem, as to whether the world and pure consciousness *actually* are as described by the above *a priori* analysis, and whether they are *actually* related in the way described by that analysis, is the task of the metaphysics which Ingarden has promised.

ANNA-TERESA TYMIENIECKA

*Europäische Philosophie der Gegenwart.* By I. M. BOCHENSKI. Francke Verlag Bern, 1951. 2nd revised edition. Pp. 323. Fr. 9.15:16s.

*Der Sowjetrussische Dialektische Materialismus (Diamat).* By I. M. BOCHENSKI. Francke Verlag Bern, 1950. Pp. 213. Fr. 8.75: 15s. 6d.

*Die Zeitgenössischen Denkmethode.* By I. M. BOCHENSKI. Francke Verlag Bern, 1954. Pp. 151. 5s.

It will not be possible to review these three excellent little books properly in a comparatively short note, but a description of their most prominent characteristics and a rough summary of their contents may at least serve to bring them to the attention of philosophical readers. All three are more or less popular expositions and display to a marked degree the virtues of their type. Fr. Bochenski's happy combination of expertise in formal logic with generally Thomist sympathies accounts for the fact that these books are at once extremely clear and lucid in expression and widely comprehensive in covering their subject-matter. They are, furthermore, remarkably instructive about matters of considerable interest to English-speaking philosophers about which it has been hard to obtain balanced and reliable information.

Fr. Bochenski's survey of contemporary European philosophy deals mainly with the period from the war of 1914 to the present day. The historical preliminaries and general intellectual background of recent philosophy are brilliantly sketched in the first chapter, where he argues with great cogency that the conceptual crisis of late nineteenth-century mathematics and physics marked the end of a recognisable phase in the

history of ideas that began in the seventeenth century. In subsequent chapters different types or schools of philosophy are considered. The work of Russell and positivism are rather oddly collocated with dialectical materialism in chapter 2 under the heading "philosophy of matter". Later chapters deal with idealism (Croce, Brunschvicg, the neo-Kantians); Bergson, pragmatism and the historicism of Dilthey; Husserl and Scheler; existentialism and, lastly, metaphysics as traditionally conceived (with particular reference to N. Hartmann, Whitehead and Thomism). The book ends with a brief but luminous appendix on mathematical logic and a very thorough thirty-five page bibliography. As is natural in a work of such scope there is a fair measure of reliance on secondary authorities, such as the voluminous Dr. Metz, at least in the treatment of subjects about which I am in a position to judge, but this is redeemed by the author's good sense and the remarkable width of his direct knowledge.

Unlike most books in English on the subject Fr. Bochenski's *Diamat* is concerned with contemporary Russian philosophy rather than with its nineteenth century German origins or even the ideas of Lenin. His attention is firmly centred in post-revolutionary Russian thinking and he deals as thoroughly with its Russian as with its Western sources. Rather less than half the book consists of an account of the history of the subject; the remainder expounds the principal theses of dialectical materialism systematically. The historical survey reveals a regular pattern, that of gradual deviation from orthodoxy abruptly concluded by the interference of philosophically illiterate intellectual policemen. The recurrent heresy is a kind of academicism which seeks to pursue philosophy without reference to the day-to-day exigencies of the class struggle. Deborin's indictment in 1931 for 'Menshevik idealism' or 'formalism' was followed by Stalin's authoritative restatement of the orthodox doctrine and a cautious inertia among philosophers. But by 1947 it was necessary for the late Comrade Zhdanov to reprove Alexandrov for heretical backsliding. In very recent years heresy seems to have revived in the writings of the 'moderates' led by Markov and Kedrov, who argue that the theories of natural science cannot be criticised from the point of view of the party's needs. But it is their orthodox opponent Maximov who seems to have received the support of the propaganda ministry. A minor peculiarity of the book is the choice of Czech transliterations of Russian names and it takes a little reflection to realise that such unfamiliar designations as Nečajev, Trockij and Vyšinskij refer to well-known people.

Fr. Bochenski's book on methods of thought embodies the most original conception of the three and is constructed with a special concision and elegance. It is an essay in methodology, which is defined as 'the study of the application of rules of logic to the practice of thinking'. This hardly fits the first method examined, the phenomenological, with its somewhat loose directives to avoid subjective associations, theoretical assumptions, the influence of tradition and concern with the existence of the object of awareness. The second method considered is the 'semiotic' method of language-analysis. Under this heading are explored the nature and limits of the type of formalistic analysis of language familiar from the work of Carnap, the syntactical rules that define such notions as 'well-formed-formula', the notion of semantic levels (i.e. use and mention) and the relations of sense and verifiability. This method is illustrated by a sketch of Tarski's theory of truth. The axiomatic method is the third discussed. After the notion of an axiom-system has been explained, its

employment in mathematical logic is examined and a chapter follows on definition and concept-formation. The axiomatic method is illustrated by a rather rigorous, Lesniewskian, formalisation in the Polish symbolism of the Hilbert-Ackermann calculus of statements. Finally, and perhaps of most interest to English readers, what we should be more inclined to call 'induction and probability' is discussed by Fr. Bochenski under the heading of 'the reductive method'. This contains the best accessible exposition I know of Lukasiewicz' doctrine, expounded in an article in Polish in 1913, that there are not two kinds of logic, deductive and inductive, but, as Popper maintained in his *Logik der Forschung*, that the single, traditional set of logical rules may be used in different ways and for different purposes. Thus both deduction and inductive explanation make use of the same relation of entailment between ground and consequence; but, where deduction seeks a consequence for a known ground, inductive explanation seeks a ground for a known consequence. In this part of the book there are chapters on the logical structure of natural science, explanation, induction, probability and historical method. In the index of names the following people are mentioned three or more times: Aristotle, Carnap, Frege, Heidegger, Husserl, Kant, Lukasiewicz, Marcel, Mill, Russell, Sartre, Tarski, Thomas Aquinas and Whitehead. This may add some weight to my claim about the width of Fr. Bochenski's sympathies.

*Note added in proof:*

Since the preceding note was written the first of the books mentioned has been translated into English by Donald Nicholl and Karl Aschenbrenner as *Contemporary European Philosophy* and published by the University of California Press (British agents: Cambridge University Press) at 37s. 6d. As the price suggests it is an inappropriately grandiose piece of book production. The appearance of a substantial treatise is given by the use of large print and wide margins. The translation is clear and accurate though rather heavily literal. 'Er entfaltete eine ausserordentlich fruchtbare schriftstellerische tätigkeit,' for example, becomes 'he has displayed an extraordinarily fruitful literary activity.' It is a pity that it was not published in a more modest, and so more generally accessible, form.

A. M. QUINTON

*Plato's Theory of Art.* By RUPERT C. LODGE. Routledge and Kegan Paul Ltd., 1953. Pp. i + 316. 25s.

THIS book is not intended as a contribution to Platonic scholarship. Professor Lodge is concerned to paint a picture of certain parts of Plato's thought. Considered as an impressionistic picture—remarks of Plato's on a wide range of topics seen from a distance in a soft light—it seems to me a fair representation. But one cannot help regretting the form in which Mr. Lodge's great erudition is conveyed. Even in a work of popularisation, some differentiation of topics is desirable; on this particular subject, since Plato seems, rather oddly, to have thought well of beauty and craftsmanship, but poorly of art, these three topics at least ask to be distinguished. But Mr. Lodge takes τέχνη as his subject, and gathers together Plato's remarks about τέχνη as if they concerned a single topic. This begs an important question. Again, the dialogues are quarried for



relevant observations without any consideration of either the date or the main preoccupation of the dialogue from which the stones are hewn. But what is most perplexing is that Mr. Lodge hardly ever draws a distinction in the text between things Plato actually said, and Mr. Lodge's own reconstruction of what underlies them. One expects a summary of the more important passages, followed by discussion and elucidation of their meaning. One never gets it; instead there is continuous exposition, in which paraphrase, probable interpretation, and pure conjecture mingle helter-skelter. No doubt the well-read Platonist could discern which is which; his less well-read colleague will be continually irritated by uncertainty, and the novice is likely to be deceived. This dangerous method is at its worst when Mr. Lodge is depicting not Plato's opinions, but those of the Protagorean humanists, Ionian scientists, and Pythagorean and Eleatic idealists, whose views on *τέχνη* are supposed by him to have provided the bricks from which Plato selected the materials for his own building. These persons are turned by an act of creative imagination into coherent schools, advancing articulate doctrines on the relevant topics. The reader must be warned that this is precarious speculation. The reader should also be warned, emphatically, about the notes. Instead of summaries of what Plato actually said, there are notes at the end of each chapter. These notes mostly consist of long *catenae* of references to the dialogues; so, the reader may well suppose, there is ample scripture to be quoted in support of the statements in the text. But if he takes these references up, he will find that some of them are (pardonably) mistakes, and, more seriously, that many of them (indeed all in some cases) merely conduct him to passages where Plato is talking about roughly the same thing as Mr. Lodge, and which have no tendency to confirm the latter's interpretation. I will give only one example, but it is one of far too many. On page 240 we are told that "Plato declares . . . without circumlocution that we apprehend beauty, 'absolute' beauty, when we look at a pure white or pure red . . ." This makes one sit up; but there is a note, and it gives five passages. Three seem totally irrelevant. Of the other two (from the *Philebus*), one speaks of "so-called beautiful colours" as providing pure pleasures, the other says that the purest white is the most beautiful white. None in fact gives any warrant for language about apprehending "absolute" beauty. There is enough of this to make the book unsafe in the hands of anybody except an expert. Finally a point of substance rather than manner. Plato is depicted as a reasonable man, given to compromise and measured statement, culling the best of other men's flowers. On this subject at least, this needs supplementing, surely, with a picture of Plato as a mischievous man, more interested in pricking other men's balloons than in making a positive contribution of his own.

I. M. CROMBIE

*Religion und Philosophie. Ihr Verhältnis zueinander und ihre gemeinsamen Aufgaben.* By FRANZ BRENTANO. Aus dem Nachlass mit Zugrundelegung der Vorarbeiten A. Kastils herausgegeben von Franziska Mayer-Hillebrand. Bern: A. Franke Verlag, 1955. 22 Swiss francs.

WHEN Franz Brentano became professor at Würzburg in 1866 he was a Catholic priest. He had turned to the Church eleven years earlier, at the age of seventeen, because of philosophical difficulties about determinism which threatened to upset his religious faith. He had then studied under

Trendelenburg—his first book is dedicated to him—but he had come to think of Aristotle as his teacher, and next to him St. Thomas Aquinas. The lectures he gave most frequently during his first three and a half years at Würzburg were on metaphysics. Carl Stumpf was one of his earliest pupils, and he tells us that by 1870 Brentano was feeling difficulties in reconciling his general account of substance with the theological teaching concerning Incarnation and the Trinity. A crisis in his thinking about religion, and eventually a break with the Church, was probably inevitable—or so it seemed to Stumpf later. But it was precipitated then, in 1870, by the controversy over the proposed dogma of papal infallibility. Brentano refers to this in the preface to his book on Jesus and Christianity (dated 1916, but published later, after Brentano's death, by Kastil, with the title "Die Lehre Jesu und ihre bleibende Bedeutung"). Like any Catholic, he had thought that doubt in connexion with the established dogmas was a sin, and he had never allowed himself to work out the criticisms he felt. But it could be no sin to examine the proposed dogma of infallibility while it was still not adopted. And his examination of it convinced him that it was false. Yet it was subsequently adopted by the Church. This led him to examine the whole of the Church's teaching, and finally to reject the Church's conception of religious faith. He left the priesthood, and later left the Church.

He was never attracted by Protestantism, and he remained outside organized religion. He could not accept any religious belief of which he could not be convinced by analysis and argument. The belief in God could rest only upon grounds which can be discovered by philosophy. He never doubted that they could be; and he thought that they were the chief business of philosophy. (His lectures on the existence of God, with an important later essay, were published by Kastil in 1929, with the title "Vom Dasein Gottes". Some of the essays in the present volume are short statements of what is discussed more fully there.)

Even so, there is a difference between religion and philosophy. But we may distinguish between less perfect and more perfect religions—as we commonly distinguish between primitive religions and highly developed religions. Brentano thought a religion was more perfect than another if its teachings were nearer to the teachings of philosophy. And from this we may imagine what an ideal religion would be. We can even imagine some of the conditions that would make it possible. And if an ideal religion did appear, then there would be no difference between religion and philosophy. People would accept the teachings of philosophy as they now accept the teachings of a church, and the authority of a religion would be like the authority of a science.

Existing religions would clearly not admit that their worth or importance could be measured in that way. For Christians or for Mohammedans, for instance, their religion is important because it brings the word of God as it has been received through revelation; and it could not have been received in any other way. It is nothing like a scientific conclusion from common evidence. It is not accepted as a conclusion is, it is accepted from other motives. This means that religious belief (at least where it goes beyond natural religion) is something different from any belief that is reached in philosophy. That is what Brentano was challenging.

So he went on to examine and criticize the whole idea of supernatural revelation, and especially that idea of belief which says that doubt is a sign of evil will. This is in many ways the most interesting part of the book, but it is unfinished. It should be taken together with the longest

section of "Die Lehre Jesu", in which he discusses Pascal's apology for Christian faith. The call for a faith that is out of proportion to the grounds for belief, does violence, Brentano thought, against the God-given command of natural reason.

By that reason we seek to understand things, and to know why they are as they are. So we are led to a first cause, which is creator of everything. It is in his treatment of contingency that Brentano makes first philosophy, as Aristotle made it, identical with theology. He argues then that the first cause must be intelligent and morally perfect; and from this, that the course of the world must always be for the best. (He thought of the world as in endless process towards greater perfection.) Finally he seeks to show that the human soul—what thinks and feels—must be spiritual and imperishable.

There may be few to accept Brentano's view of philosophy now. And fewer still to see why a general trust in such philosophy should be called religion. But the book is about the relation between philosophy and religion, and the very strangeness of its conclusions is often what makes it worth study. Brentano writes from difficulties that he has known, and he does not write for effect. And what he has to say should at least be answered.

R. RHEES

*The Contemplative Activity.* By PEPITA HAEZRAHI. George Allen & Unwin, 1954. Pp. 139. 12s. 6d.

DR. HAEZRAHI takes as the basis of her enquiry into aesthetic appreciation the existence of aesthetic experiences. Her central thesis is that such experiences are intrinsically non-emotional and contemplative; from this she develops a theory of the critic's and of the artist's activities, regarding the former as assistance to the objective evaluation of aesthetic experiences, the latter as a practical problem-solving activity, not necessarily connected with the expression of emotion.

What aesthetic experiences are, Dr. Haezrahi never clearly tells us. She uses two methods of getting us to recognize them. The first lies through introspection, and she offers a phenomenological description of them in terms of intense concentration, sense of distance, and so on. Second, she offers a method of proof, and suggests that the mere fact that we can have sensible discussions about beauty, which are distinct from other types of discussion, demonstrates the existence of the appropriate type of experience. The second method is in itself suspect; in any case, it is not easy to see how both methods can be employed at once, without meeting a familiar type of objection. Either the criteria of a sensible discussion about beauty include a reference to the occurrence of the experience that has been identified phenomenologically, or they do not. If they do, the argument is circular, and establishes no independent conclusion. If they do not, then either the two approaches lead to different types of conclusion, in which case the existence of no one type of experience has been established by them both, or they lead to conclusions connected synthetically and *a priori*, so that one set of (linguistic) events will entail another set of (psychological) events—which seems unpalatable.

The aesthetic experience is said to be intimately related to utterances such as "this is beautiful". These have a double function. Objectively, they designate the experience, or "designate the fact that the experience

has occurred"—even though the word "this", in "this is beautiful", refers to an object and not to an experience. Subjectively, they mark or express the spectator's reaction to the experience or to what is grasped in the experience. Thus the aesthetic experience turns out to have a *Doppelgänger*, in the form of the spectator's reaction or response to it, which possesses features denied to the experience itself. Dr. Haezrahi emphasises, for instance, that the aesthetic experience as such is to be distinguished both from an emotional reaction to an object and from a judgment on it. Yet the emotional reaction seems to be divided off from the experience itself only by the shadowy partition of the subjective-objective distinction. The function of judgment is no farther away, since the reaction or response is said to be "borne on a strong flow of approval and satisfaction". But if this is so, and there is always a response when there is an aesthetic experience, it follows that with an aesthetic experience there always go feelings of satisfaction, and the purity of the experience itself is maintained only by a verbal device. We are left, moreover, with the question of whether the recognition of the ugly involves an aesthetic experience; it follows from the argument that the answer should be 'no', but in fact the author seems to give wavering advice (p. 20 against p. 13).

Outside her ontological box-room, Dr. Haezrahi's house is in better order. She has stern things to say against peddlers of sentiment, and raises the interesting question of the supposed uniqueness of aesthetic perception contrasted with some kind of generality contained in emotional reaction; what she says is suggestive but not developed. The construction of works of art is analysed in terms of problem-solving, although the kinds of problem involved are necessarily left rather vague. The concepts of form and content are stoutly if rather mysteriously commended: "critics . . . must of necessity operate with the logical terms of content and form, however defined. . . ." The author has some interesting things to say about the different use by artist and spectator of these concepts.

The book consists of eight lectures, and from the lecture room some irritating prolixities and condescensions remain. Surprisingly, Collingwood's name twice appears in footnotes with the initials 'R.C.'

Dr. Haezrahi has sought to trap the bright beasts of aesthetics by drawing new creepers through the undergrowth. Perhaps if she had more consistently tried to clear a small space, her sensibility would have made a more substantial capture than she has in fact achieved.

B. A. O. WILLIAMS

*The Phenomenology of Moral Experience.* By MAURICE MANDELBAUM.  
The Free Press, Glencoe, Illinois, 1955. Pp. 338. \$5.00.

THE main thesis of this book may be regarded as falling into three parts, dealing with the methods of ethical enquiry, the analysis of moral judgments, and the resolution of moral conflicts.

Professor Mandelbaum examines various approaches to the study of ethics, and holds that only one approach can help us to resolve moral disagreements. This is a form of phenomenological approach which he calls both a 'generic' and a 'structural' approach. As a generic approach, it tries to discover what all moral judgments have in common, while, as a structural approach, it sets out from the fact that persons make moral

*judgments*, and tries to discover the relationships between such judgments and the circumstances under which they are made.

He distinguishes three kinds of moral judgment. These are: (1) "direct judgments of moral rightness and wrongness", or the judgments of an agent on his own present or prospective actions; (2) "removed judgments of moral rightness and wrongness", or the judgments of an observer on the actions of another (or of an agent on his own past actions); and (3) "judgments of moral worth", which are judgments commending or condemning a person's character. All these, he finds, have one structural characteristic in common. When they are made, the attendant situation is experienced as giving rise to an *objective demand*, and the ground of the judgment is found in the apprehension of a relation of *fittingness* or *unfittingness* between a person's responses and these demands.

He examines the sources of moral disagreement, but his most important contribution here concerns the ways in which such disagreements may be settled. He distinguishes between the *validity* of a moral judgment and its *truth*, and argues that while we cannot establish the truth of any moral judgment, we can show many to be invalid, and may reject them for that reason. The validity of a judgment is a function of the way in which it comes to be made, and is subject to three different tests. (1) Any judgment, to be valid, must be grounded on the facts of the actual situation. (2) All judgments must be consistent, not in what they assert, but in the method by which they are reached. (3) Any judgment which is valid on the first two principles is incorrigible, which implies that any judgment which seeks to correct such a valid judgment is itself invalid. The conclusion finally reached is that many moral controversies are soluble on one or other of the first two principles, but that the third principle places a limit on what can be achieved in this way. Where two persons are convinced that their judgments are valid, there is no means of resolving their disagreement, and a moral *impasse* is unavoidable.

This is a somewhat novel thesis. In intention, it is a revolt against the tendencies of logical analysis, now dominant in Anglo-American ethical theory. While one may sympathise with the spirit of the revolt, the question is whether the revolt succeeds. With some regret, I must say that it does not seem to me to do so.

The classification of moral judgments reveals one obvious difficulty. Let us take judgments of moral worth. These are all regarded as removed judgments. Yet there can be no doubt that persons make judgments about their own characters. More important, such judgments are difficult to reconcile with the present thesis. It is difficult to find an awareness of an objective demand associated with judgments of this kind, since we usually claim to be honest and truthful, etc., only when our honesty or truthfulness is being impugned, not when we are confronted with situations which require us to demonstrate these virtues. For the same reason, it is difficult, when we make such judgments, to find an awareness of a relation of fittingness between these traits of character and the demands of any situation which is present at that time. The author's safeguard against this or any similar criticism is the assertion, made boldly in advance, that "it is only by distancing ourselves from our actions, observing them as we would the actions of another, that we can judge what our own moral character may be" (p. 135). This enables him, without further ado, to assimilate all judgments of this kind to removed judgments, and to attribute the structural properties of the one to the other. Obviously this is a *petitio principii*. But it is by such

means that the generic and structural approach is brought to a seemingly successful conclusion.

A more serious difficulty appears if we examine the distinction between validity and truth. (It is a loose use of terms to ascribe validity to the judgment rather than to the method by which it is established, but that is a minor point.) Let us grant that some such distinction is necessary. The question then is this. To what extent, if any, do the methods prescribed by Professor Mandelbaum guarantee the truth of our moral judgments, or, to what extent does the infringement of the methods guarantee that our judgments are false? Unless there is some logical connexion between validity and truth, then valid judgments need never be true nor invalid ones false, and the point of the distinction is entirely lost. Professor Mandelbaum nowhere sees or meets this problem. But that is not the most serious matter. If in the interests of the distinction we try to meet this problem, we get into conflict with the remainder of the present thesis. An enquiry into the relationship between validity and truth would have to be a logical one. It could not therefore fall within the confines of a purely phenomenological approach to ethics. In short, it is difficult to avoid the conclusion that the thesis as a whole is fundamentally incoherent.

Even so, it is still difficult to dismiss this as a negligible book. What Professor Mandelbaum has to say about the resolution of moral conflicts, for all its deficiencies, seems to me to be important, as do several other matters which are more incidental to his main theme. Among the latter I would especially include the phenomenological criticism of the doctrine of *prima facie* duties (pp. 73-83) and of teleological theories (pp. 98-109), as well as the whole section in Chapter III dealing with the phenomenology of moral attitudes (pp. 115-126). When Professor Mandelbaum is content merely to describe the phenomenological features of moral experience, his insights are good, and his analyses often carry, as it seems to me, an overwhelming conviction. This is the value of his book. It is all the more regrettable that he did not do this work straight-forwardly, without attempting to turn it into a methodological weapon for revolutionising moral philosophy.

Professor Mandelbaum has some predilection for a vague and old-fashioned terminology, and is much lengthier than he needs to be, but he writes vigorously at all times, and in a charming controversial style.

W. J. REES

*Godwin's Moral Philosophy.* By D. H. MONRO. Oxford University Press, 1953. Pp. 205. 15s.

WRITING with moderation and good humour, Mr. Monro sets out to defend Godwin against those who misinterpret his doctrine and by so doing make him out to be a fool. One of Godwin's great merits was that he was a reformer who believed passionately in the possibility of change for the better. But by a series of philosophical manoeuvres with the concepts of 'voluntary action', 'desire', and 'thinking a thing good', he managed to convince himself that men could be trusted to do what was for the best if they could see it to be best, and to see it to be best if only they could see the facts clearly; hence the war on ignorance and prejudice, and on such things as titles, which fostered fictions in men's minds. Mr. Monro points out that this cannot be seen as an unqualified

rationalism, since for Godwin 'seeing the facts clearly' came almost to be defined as feeling the right emotions, and in particular as having that proper sense of the sufferings of others which would remove partiality for oneself or one's friends. It was his uncompromising, though not entirely consistent, utilitarianism, together with his belief that the rational man could not stop short of universal benevolence, that led him to his notorious verdict in favour of the Archbishop in the "famous fire case, Fenelon versus my father the valet". And it is this type of rationalism (in the popular sense) that has given Godwin the reputation of a cold-blooded monster and a crank. He has hard things to say, for instance, of the prejudice which hides the faults of husband from wife, or child from mother, and he thought that men should frankly point out their fellows' faults. In fact Godwin had an almost absurd belief in the efficacy of the naked truth, but one feels that the absurdity of some of his conclusions would easily have been forgiven him if he had possessed more literary and imaginative power. As it was he briefly, but tremendously, inspired the radicals of 1793, and has now been almost forgotten except as Shelley's father-in-law.

Mr. Monro concerns himself with defence, and with exposition. The first is the less interesting, since he here bowls along easily on phrases such as "tight-lipped morality", and "a bloodless reason", which tend to dull the ear to both prosecution and defence. But the chapters in which Mr. Monro traces Godwin's conclusions back to the assumptions from which he started are excellent. Finally, the criticism, of which there is not a great deal, is shrewd and lucid, and does not pretend to be profound. As Mr. Monro has succeeded well in what he set out to do, it is perhaps churlish to suggest that more philosophical interest might have been extracted from Godwin's writings. But as he did not confine himself strictly to exposition it does not seem unfair to ask him to wrestle with Godwin where he is easy to dismiss but difficult to answer. What, for instance, is wrong with his verdict in the famous fire case? It seems as ridiculous to give good reasons why one ought to save one's father the valet as it is to accept Godwin's judgment. On this matter, and on others, such as the impossibility of reconciling a belief in determinism with the usual ideas of innocence and guilt, Godwin might do us the characteristic service of the man of radical temper who refuses to dismiss arguments because they lead to curious conclusions; but of course Mr. Monro is right in his unspoken assumption that Godwin's work is mainly of historical interest.

PHILIPPA FOOT

*A History of Chinese Philosophy.* By FUNG YU-LAN, translated by DERK BODDE. London: George Allen and Unwin, Ltd. (Princeton, N.J., U.S.A.: Princeton University Press.) 2 vols. Pp. xxv + 455, map; xxv + 783. 40s., 55s. net.

GREAT philosophies anciently arose in each of the three centres of high civilization. Each centre developed distinctive characteristics. The classical European philosophy was dominated by metaphysics. Thales began thus by asking what the world is made of. Indian philosophy was dominated by the problem of salvation, posed by the miseries of life. Chinese philosophy, started by Confucius, turned to ethics and political problems. In each area, this dominant feature persisted. Yet the



assumptions underlying the dominant problem led to other problems. In India, salvation, i.e. the problem of orienting life in accordance with the nature of the universe, inevitably led to metaphysics.

In China, the high altruism of Confucius, together with the facts of human conduct, raised the problem whether human nature is adaptable to that ideal. Mencius eulogistically taught that human nature is naturally good. But Hsün-tzu saw that the Confucian ethical authoritarianism (like Catholic Christian dogma) secures a firm foundation only when it is taught that human nature is fundamentally evil. Then Confucian teaching derives from all-wise authorities, the Sages, who are the sources of secure knowledge. Thus there developed what Kant called the metaphysics of ethics.

But the Chinese *enfant terrible*, the Lao-tzu, writing about 300 B.C., took away the foundation from beneath all Confucian teaching by arguing that the universe is fundamentally antagonistic to any altruistic ethics. In that period of constant warfare, as in similar epochs since, this contention appeared highly plausible. Then Confucians were driven to formulate their own metaphysics. Tung Chung-shu (the Chinese put surnames first) in the second century B.C. developed such a system, which assimilated much of what was really Chinese neo-Pythagoreanism. So Confucianism became a non-theistic legalism. In the twelfth century A.D., however, Chu Hsi, one of the world's great metaphysicians, profiting from Buddhist and Taoist metaphysics, reformed Confucian metaphysics. His monistic dualism deserves to be ranked with Aquinas in its scholastic detail, its completeness, and its continued dominance in Chinese thought. In these and other ways, Chinese philosophy developed differently from European thought. Anyone who wishes to understand, not merely the particular ideas current in Europe, but the nature of philosophy in general, must become familiar with the development of thought in all three centres of philosophy, for each shows its own pattern.

Fung's work is our first really satisfactory history of Chinese thought. It is a truism that rarely can anyone who is not a creative philosopher successfully comprehend and expound for the first time, the thought of other great philosophers. Fung has to his credit five volumes of his own philosophy, one of which is translated by E. R. Hughes as *The Spirit of Chinese Philosophy*. Chinese philosophy is, moreover, hampered by being expressed in the very difficult Chinese classical style. One who can read between the lines and see the problems being discussed in unfamiliar terminology will find this book stimulating indeed. This English translation was made with the collaboration of the author and is beyond praise. The translator has added many notes, appendices, and bibliographies, which help the reader greatly. For those not thus interested in multifarious details, Fung has written a briefer work, *A Short History of Chinese Philosophy* (edited by Professor Bodde).

To name only a few of the many merits in this work: It rightly elevates Tung Chung-shu as the authoritative theologian of Han Confucianism and correctly treats Wang Ch'ung as a minor figure. The latter is much better known and esteemed in occidental circles because there is for him a good English translation! Fung's account of Chinese Buddhist philosophy and its relation to medieval Confucianism is original and overcomes the serious difficulty of Buddhist Chinese terminology, which is incomprehensible even to most literate Chinese. The many originalities of early medieval Neo-Taoism are ably expounded. With this thought Fung is deeply sympathetic.

For occidental readers, this work is hampered by the circumstance that it was originally written for Chinese students. It frequently quotes Chinese texts. Since, however, even to educated Chinese, these texts are difficult, this book accordingly expounds their meaning at length. As, however, a good English translation of the original is itself an explanation, we find in this translation easily comprehensible original passages followed by prolix explanations thereof! This work, like large histories of European philosophy, deals with many minor philosophers, who are often merely curiosities of history. The Confucian canon contains much scholastic verbiage, some jejune metaphysics, and a good deal of ancient science and superstition, all of which were historically influential, but of little philosophic interest. They are duly recounted and expounded.

My chief criticism of this history is that Fung, like most Chinese, does not do adequate justice to the philosopher Mo-tzu. I believe him to be the most original of all Chinese thinkers. At times I wonder whether he does not actually surpass Confucius in seminal influence. But Mencius, who was a much greater literary artist, adopted without acknowledgement many of Mo-tzu's ideas and slandered him very effectively. Consequently few, except original thinkers like Han Yü, have since bothered to read this ancient philosopher. His influence passed greatly into ancient Confucianism and is found in some of its classics. Indeed, Confucianism triumphed because it could adopt and use the best ideas of its rivals—Mo-tzu, Buddhism, and Taoism—so that Chinese minds naturally accepted the most comprehensive philosophy.

HOMER H. DUBS

*Clinical Versus Statistical Prediction.* By P. E. MEEHL. Minnesota University Press (London: Geoffrey Cumberlege). Price 24s. net.

THIS is a work of applied philosophy. In it Professor Meehl sets out to answer the question whether "clinical" or "statistical" methods of predicting human behaviour are the better. He uses the word "clinical" in the broadest sense to include all methods of prediction which involve the framing of hypotheses about the dynamics of particular situations—our usual methods of predicting the behaviour of our acquaintances would be one type of clinical prediction in this sense, though of course the psychologist may make use of hypotheses and constructs which have no parallel in everyday language. Statistical predictions are those made by assigning an individual to one or more classes, and then making use of the frequency of the occurrence of the event to be predicted for those classes, combining the classes by some more or less sophisticated statistical device to yield a prediction for the individual case. Meehl attempts a detailed analysis of what the question means before giving his answer: in the course of this analysis he draws many important distinctions, e.g. he points out that in data collection mathematical or non-mathematical techniques may be employed and that this involves issues different from the issues raised by the use of statistical or clinical methods for combining any given set of data in order to arrive at a prediction.

It is Meehl's analysis of what is involved in the two methods of prediction which will be of most interest to philosophers. At this point a number of philosophical questions concerning scientific method meet: e.g. whether there is a difference between "historical explanation" and

scientific explanation, whether there is more than one kind of probability, whether rules for induction can be formulated, and so on. While Meehl does not throw any blinding new light on these problems, it is interesting to see them discussed with a specific reference and he produces many fascinating examples in working out his analysis, and exposes very clearly some of the major difficulties in giving detailed scientific explanations of behaviour. For example, he points out that the way in which we categorise responses may vary not with any variations in the responses themselves treated as physical events but with how we interpret the situation in which they are made.

On the major issue of how far clinical methods of prediction reduce to statistical with the laws, class frequencies, etc., not made explicit, Meehl seems unable to make up his mind with certainty. He does, however, succeed in demonstrating the practical difficulties in the way of reducing one to the other, and he removes the air of mysticism which sometimes surrounds the activity of the clinician by giving ingenious examples of the kinds of laws which must be operative if the clinician's predictions are to have any justification: his examples could be applied, *mutatis mutandis*, to the kinds of explanations and predictions we give of behaviour in everyday life.

Meehl's analysis of what is involved in the dispute between clinical and statistical methods leads him to the conclusion that the dispute cannot be resolved by *a priori* arguments, and his last two chapters are devoted to a survey of the evidence bearing on the question which in fact does better. His analysis has, however, not been in vain, since it brings into clearer focus what are the differences and similarities between the different methods of prediction, and the examples he chooses to illustrate his points are fresh and highly illuminating.

N. S. SUTHERLAND

*Ethics in Theory and Practice.* By THOMAS E. HILL. New York: Thomas Y. Cromwell Company, 1956. Pp. xvi + 431. \$4.50

THIS handbook of ethics for "the student" is not very likely to find many buyers in this country, where not much worse ones are available at a fraction of the price. The first 200 pages are devoted to a survey and criticism of the usual types of ethical theory. In the next fifty the author expounds his own "working theory", which differs from that of G. E. Moore only in detail and in the insistence that only "conscious experiences" are ever "intrinsically good". The rest of the book claims to elicit the "practical ethical principles" implicit in this theory, and the student of ethics finds himself committed philosophically to anti-communism, promotion of world government, etc.

The most important developments in recent moral philosophy are ignored. "The Emotive Theory" is criticised as if nothing had been written since *Language, Truth and Logic*. The one exception is a single reference (un-indexed) to Mr. Hare, who will (I hope) be suitably indignant at being cited in support of the view that the meaning of "good" is a "valuational characteristic" (pp. 213, 219).

At the end of each chapter the student is referred to one or other of the books of "readings" in ethical theory. Original texts are also listed, but separately, for the sake of "special studies". Questions are set, of which

the following is quite typical : " How do you think Ayer would propose to resolve the current moral differences between East and West ? What role would reason play in this ? How about oratory ? Is Ayer's type of solution for these problems satisfactory ? Explain."

BERNARD MAYO

Received also :

- American Philosophers at Work*, ed. S. Hook, New York, Criterion Books Inc., 1956, pp. 512, \$7.50.
- From Aristotle to Plotinus*, Selections in Translation, ed. T. V. Smith, Chicago University Press, Phoenix Books (London : Cambridge University Press), 1956, pp. viii + 293, 12s. 6d.
- A. J. Ayer, *The Problem of Knowledge*, London, Macmillan & Co. Ltd., 1956, pp. x + 258, 18s. Also in Pelican series, pp. 224, 3s. 6d.
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- K. Bhattacharyya, *Studies in Philosophy*, Vol. I, ed. G. Bhattacharyya, Calcutta, Progressive Publishers, 1956, pp. xiv + 366, £1 10s.
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## IX.—NOTES

### COUNCIL FOR ASSISTING REFUGEE PHILOSOPHERS

In the year 1939 the chief philosophical societies in Great Britain set up this Council, under the Presidency of Lord Samuel, for assisting refugee philosophers from the continent. A small fund was raised and this was administered through the Society for the Protection of Science and Learning. The Council was also instrumental in making provision for some of the activities of the Brentano Institute to be continued in this country, and in facilitating the arrangements whereby Professor Oscar Kraus was enabled to deliver his Gifford Lectures in Edinburgh in 1940-41.

After the outbreak of war responsibility for the activity of the Council was vested in a small Executive Committee consisting of Professor C. D. Broad (Chairman), Professor G. E. Moore, Professor M. Ginsberg and C. A. Mace (Secretary and Treasurer). This committee transferred the balance of the funds of the Council to the Society for the Protection of Science and Learning except for the small sum of £26 which was retained in case circumstances should arise which might call for a revival of activities on the part of the Council as such.

It has now been decided by the Executive Committee that any further action that might be called for would best be taken by some larger and more central organisation. Accordingly, with the agreement of representatives of the associated societies, the Committee has now transferred the balance of its funds to the Society for the Protection of Science and Learning, earmarked for use in the interest of refugee philosophers. It is understood that the money can be used to assist refugee philosophers from Hungary.

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### ERRATUM

The reviewer of "*The Structure of Metaphysics*", by M. Lazerowitz, regrets that he omitted to mention that the book has a foreword by John Wisdom.



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